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Factorial covariance analysis within a decision framework : theory and application.

James Michael Clapper
University of Massachusetts Amherst

FACTORIAL COVARIANCE ANALYSIS WITHIN
A DECISION FRAMEWORK: THEORY AND APPLICATION

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James Michael Clapper

Approved as to style and content by:

Donald G. Frederick (Chairman of Committee)

Carl Dennler, Jr. (Acting Director of Graduate Studies)

Richard J. Giglio (Member)

Gordon W. Paul (Member)

Parker Worthing (Member)

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Factorial Covariance Analysis within a Decision Framework: Theory and Application (October, 1973)

James Michael Clapper, B.S., Rensselaer Polytechnic Institute
M.S., Rensselaer Polytechnic Institute

Directed by: Dr. Donald G. Frederick

This research is concerned with the methodology for analyzing designed experiments within the framework of Bayesian decision theory. The more widespread use of experimentation in applied marketing research is restrained in part by the inadequacies of traditional analysis procedures, which very often fail to clearly indicate the correct alternative in a decision situation and which also fail to provide adequate guidance on the question of total sample size for an experiment. Bayesian decision theory offers relief from these obstacles but the development of suitable methodology must precede any application of these concepts. In this research, a Bayesian methodology for analyzing data from a factorial design with covariates is developed by expanding upon Schlaifer's work with the concept of differential utility. The practicality of the developed methodology is demonstrated by applying it to data gathered to aid a firm in a product-assortment decision. Suggestions are also made for using the same general framework employed in this research to develop Bayesian procedures for other experimental designs.

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CHAPTER I

THE MARKETING MANAGEMENT NEED FOR IMPROVED EXPERIMENTAL ANALYSIS PROCEDURES

Introduction

This research is concerned with the methodology for analyzing designed experiments within the framework of Bayesian decision theory. As outlined below, there is a need for a decision-oriented methodology of analysis that can be applied to data from experiments conducted in a marketing management context. This research seeks to contribute to that needed body of techniques. Here the work of Schlaifer[1] and others with the concept of differential utility is extended to yield a Bayesian methodology for analyzing data from a factorial design with covariates. The methodology is then applied to data from an experiment that was conducted to help solve a product-assortment decision problem faced by the management of a retail food chain. Although the matter is not pursued in depth in this research, the same principles used in developing the methodology presented here could be used to develop techniques of Bayesian analysis for other experimental designs. Some comments on how this might be accomplished are made in Chapter IV.

Prior to the discussion in later chapters concerning the development of the methodology and its application, the remainder of this chapter is devoted to reviewing the need marketing management has for improved techniques for analyzing experimental data and to outlining the background of the experiment that served as an example application.

Experimentation in Marketing

Importance of Experimentation

Controlled experimentation has become an accepted research technique in the field of marketing. One author reports that between 1955 and 1968, over 100 marketing experiments were reported in the literature.[2] Since that time there have certainly been at least another 100 reported and there is no way of estimating how many experiments have gone unreported for proprietary and other reasons. Reported experiments have involved virtually every facet of marketing from product design to choice of advertising appeals. Despite this encouraging beginning, experimentation still constitutes only a small percentage of all marketing research projects. As marketing strives to become a more scientific discipline, however, further growth of experimentation is to be expected.

Controlled experimentation is a cornerstone of the scientific method of gaining knowledge.[3] Experiments are designed to test explanations of phenomena--to determine cause and effect relationships. Experimentation is preferred to other research forms because its unique virtue of control makes it possible for experiments to provide more than just evidence of correlation and time order of occurrence. Controlled conditions allow experiments to provide evidence ruling out other variables as possible causes and thus provide the researcher with confidence that the discovered relationships are causal, not merely correlational. This advantage of experimentation in determining causal relationships is important and serves as a strong motivation for increased application of the experimental method within the discipline of marketing.

Impediments to Experimentation

Although the desire for increasing the proportion of experimental research in marketing exists, there are a number of factors that serve to hinder the more widespread use of experimental methodology. There are, of course, the kinds of factors that serve as obstacles to any systematic research in marketing, factors such as the complexity of studying humans and human reactions and the wide-ranging problems of measurement. In addition, those who would conduct experimental studies must also cope with time and cost problems more severe than those involved with other research designs. They must also cope with problems of control over relevant variables and administrative problems in establishing and maintaining experimental conditions.

Experiments take time to design and perform, especially when the experimental variables are thought to have some delayed effects upon the criterion variable. Given the dynamic nature of marketing phenomena, the utility of the information from an experiment may begin to evaporate before the experiment is even completed.

Experiments are often more costly than other systematic data-gathering techniques. The need for control, the amount of time involved in experimentation and the requirements of pre- and post-measurement all serve to make experimentation more costly.

Time and cost considerations aside, there are still control problems to be overcome. Many variables, such as weather and competitive reactions, are beyond the manipulative control of an experimentor. When possible, experimentors rely on matching, randomization and other means of statistical control to cope with these kinds of variables. In some situations, however, the nature of the variables involved or the number of experimental units needed for statistical control make the exercise of adequate experimental control physically impossible. Even where control is possible, administrative problems may thwart the experimentor's best efforts. Securing and maintaining the cooperation of a large number of people in an effort to preserve the prescribed experimental conditions can be a very imposing problem.

The Deficiency of Traditional Analysis

In addition to all of the above problems, there is another obstacle to the application of experimental methodology in a marketing management context. This obstacle is the fact that traditional methods of experimental analysis do not adequately deal with decision makers' problems of choosing among competing courses of action. In basic or "pure" research, the goal is to extend the boundaries of knowledge in a given area with no necessary regard for immediate application of the research findings to existing problems. In a marketing management context, however, research is often carried out to aid in the solution of specific marketing problems. In this context, the ultimate purpose of experimentation (or any research) is to reduce the economic risk of making an incorrect decision.[4] Traditional methods of experimental analysis are inadequate in this context. "Truth" may well be the objective in pure research but it is not the primary issue in a marketing management problem. The marketing manager is not interested in the probability of an incorrect choice of explanations for some phenomenon (Type I and Type II errors) but rather he is interested in the economic risk involved in choosing one course of action over another. Traditional methods of analysis provide no direct answer to the question of economic risk.

Another shortcoming of classical analysis within a decision context is the inadequate guidance given for determining the total size of an experiment. In situations where it is possible to gather data sequentially over time, classical methods do not provide an adequate criterion for determining at any point in time whether it is economically wiser to make an immediate terminal choice among alternatives or to postpone such a decision until after more data have been gathered. According to the classical school of thought, a sequential decision rule or sampling plan is evaluated by examining the probabilities of Type I and Type II errors that would result from its application. There is no formal consideration of the consequences of a wrong decision nor of the costs of additional sampling. As a result, the decision maker is left to employ his own informal criteria for choosing among competing decision rules. Further, it is interesting and worth noting that, strictly speaking, the significance test approach used so often in experimental analysis is, according to classical thought, inappropriate in a sequential sampling situation. The decision rule involved in a significance test involves (1) specifying the maximum conditional probability of a Type I error (α) and then (2) deciding between the competing hypotheses in such a fashion as to minimize the probability of a Type error, subject to condition (1).[5]

If a researcher tests the significance of an observed sample at some specified α, there will be a probability, α, that this first test will lead to a Type I error. If, however, the null hypothesis is not rejected and the first test is followed by another sample and test, there will be an additional risk that this second test will result in a Type I error and, thus, the total risk of a Type I error for the two tests will be greater than α. This would be in violation of the significance test decision rule. Yet, ". . . tests of significance continue to be used as the standard procedure for deciding whether or not judgment should be suspended and more evidence collected before a terminal decision is reached despite the fact that this means that the conditional probabilities of wrong terminal acts are completely unknown."[6] This state of affairs certainly does nothing to ease the decision maker's burden.

A Proposed Remedy

The shortcomings of traditional experimental analysis in the context of marketing decision making have been recognized by several authors.[7] In theory there is an alternative method of analysis that can overcome the shortcomings of classical hypothesis testing. Analyzing designed experiments within the framework of Bayesian decision theory can provide direct information on the economic risk associated with the selection of one experimental treatment over the competing ones. Such an analysis can also in theory evaluate the utility of experimenting further before making a terminal choice among alternatives. Why then is Bayesian analysis of experimental data not more prevalent in marketing? The reason seems to be that "Experimental designs using Bayesian statistics have not been developed as yet to nearly the extent that traditional designs have."[8] This dissertation is aimed at this methodological gap. Here the methodological work of Schlaifer, Raiffa and Schlaifer, and Frederick utilizing the concept of differential utility analysis is extended to provide a procedure for analyzing data from a factorial experiment with covariates.[9]

The actual company situation outlined below provided a practical motivation for seeking to extend the Bayesian methodology for designed experiments. In this research, attention was focused on developing analysis procedures for the type of experiment that seemed called for by the company's situation. The methodological results obtained, however, are not limited to the situation investigated but rather may be applied whenever this particular experimental design appears appropriate. Further, it should be a manageable task to use the same general framework employed here to develop Bayesian procedures for other experimental designs.

The Background of the Experiment

The Marketing Strategy of the Firm

The company involved in this research characterizes itself as a non-franchising, vertically integrated company. For 37 years the firm's principal business has been the operation of a chain of ice cream and sandwich shops. At the start of fiscal 1973, the chain consisted of 317 stores in eleven states. More than 150 of these stores opened in the last five years. Combined retail sales for 1972 were in excess of $60,000,000. Historically, the main thrust of sales and profit growth for this chain has resulted from a combined strategy of market penetration and market development,[10] implemented by the opening of new shops. The present target is 45 new stores per year. While this strategy will continue to account for the major growth of the chain in the immediate future, the firm has of late also become interested in increasing per store sales and profits for individual shops in the chain. This concern is motivated not only by a desire to directly increase the company's return on investment but also by the conviction that higher per unit returns are necessary to attract and hold well-qualified personnel for store manager positions. This latter consideration arises from the method by which store managers are compensated. Their income is based on a percentage of store profits. Consequently, increasing per store sales and profits is a straightforward way to increase the earnings of a store manager. Hopefully, higher earnings will make the store manager position more attractive, thereby reducing turnover and making it easier to attract well-qualified people.

One action the firm has taken, in its attempt to improve unit performance, is to expand its product assortment.

The Limited Menu Concept

Traditionally, the chain has operated with the philosophy of a limited menu of high quality items offered for sale at a price intended to give "good value" for the consumer's collar. By adhering to the limited menu concept, the firm has in the past been able to manufacture or process all the products it has sold and, thus, has insured itself of good quality control. Also, producing what it sells means the firm eliminates the marketing costs incurred by outside suppliers that would normally be reflected in higher retail prices. In this fashion the firm has protected its quality, good value image with consumers.

In recent times, to improve the performance of individual shops, the company has begun to move away from the limited menu concept by selectively expanding its list of product offerings. Nevertheless, the requirement remains that any new products added must still fit the chain's tradition of high quality and good value. Menu expansion has meant the addition of some products that are not manufactured by the firm. In these cases, though, the firm has been very cautious in assuring itself that suppliers can and will provide top quality products.

The Potential of Chocolates

In its search for new products, the firm has become interested in the possibility of adding fancy boxed chocolates to its product mix. The firm's interest in boxed chocolates results from the following considerations:

1. Quality boxed chocolates are a high margin item.
2. This item can be merchandised in a comparatively small amount of space.
3. The firm could undertake manufacture of chocolates without an unduly large capital investment.
4. Many skills used in the production of other of the firm's products are transferable to candy production.
5. This type of product appears to fit well in the operations of other retail food chains.

Because of the high retail margin that quality boxed chocolates provide, the firm feels that this type of product might be profitably sold by the chain, even if the product must be purchased from outside suppliers. The acceptability of an outside supplier, of course, will depend upon his ability to meet the firm's quality standards.

Merchandising space is an important consideration in existing shops. Most of these shops were designed for efficiency of operation before the firm began to broaden its product line. As a result, display and storage space is at a premium in existing shops.

Should sales volume warrant, it is felt that it would be feasible for the firm to produce the chocolate products it sells. The capital investment necessary is not a deterrent. In addition, the manufacture of chocolates requires many of the same skills as are used in the manufacture of such products as ice cream, syrups and toppings--products presently produced by the firm. Undertaking chocolate production, then, would not mean the firm was moving into a foreign area in which it had no manufacturing expertise. It should be noted, though, that the firm does not intend to initially undertake production. This consideration is more long-term in nature.

Finally, the firm has noted the apparent success other retail food chains have had with boxed chocolates. Competitors have not revealed any conclusive data, but discussions with trade suppliers indicate that some food chains find chocclates a very attractive product offering.

Preliminary research was carried out by the company to determine the availability of fancy boxed chocolate products that would be consistent with the quality and value standards of the firm. This effort yielded three different products which were deemed acceptable on these criteria. Considering all the evidence to date, management feels boxed chocolates could be a product compatible with the firm's objectives. The question remaining is whether or not boxed chocolates can be a profitable product for the chain. The firm has no experience in marketing boxed chocolates, and this product is different enough from any of the offerings in the existing product mix that management is unsure of what response to expect from customers. Before proceeding with chainwide adoption of a product, then, management wants more information concerning the potential profitability the firm can expect from a boxed chocolate product.

Bayesian Experimentation for Profitability Analysis

In the past decade, three points have become generally accepted by those writing about the profitability analysis stage of the new product adoption process. First, one should not attempt to estimate the profitability of new products without regard for variability in other elements of the marketing mix. There should not be one sales and profit estimate for product X but rather a family of contingent estimates, conditioned upon the firm's decisions with regard to price, advertising support, and the other elements of the marketing mix.[11]

Second, estimates of expected returns are just that, estimates. Because of the uncertainty involved, one needs not only to estimate the expected returns from a venture but also to quantify the risk associated with the decision to accept or abandon the potential new product.[12] And third, at any point in the analysis, the choices are not limited to accept or reject; there is a third alternative of postponing the terminal decision until more information is available from additional research.[13]

If a profitability analysis procedure is to be adequate, then, these three concepts must be operationalized. The procedure should allow for the evaluation of alternative marketing mixes, should provide a measure of the risk involved with the decision and should incorporate methods for determining whether or not the postponement of a terminal decision until further information is gathered is economically justified or not. A test marketing program of controlled experimentation properly conceived and executed, and combined with a Bayesian methodology for analyzing the resultant data, can satisfy these three objectives. As noted already, controlled experimentation offers the researcher confidence that the discovered relationships are causal, not merely correlational. Further, field experimentation can lead to data generated under conditions similar enough to those in "real life" to engender enough confidence in decision makers that they will be willing to truly let their decision be influenced by the experimental findings. The sentiments of many are summarized in the statement "The only way a manufacturer or distributor can <u>really</u> know whether consumers will buy the product is to offer it for sale."[14] Finally, the use of a Bayesian analysis should provide useful, decision-oriented information from the experimented data. The combination of experimental control, actual market ("real life") data and a decision-oriented analysis should have particular appeal to management.[15]

The concept of Bayesian analysis of test market data is not new.[16] The notion of controlled experimentation by retailers is not new either.[17] To this researcher's knowledge, however, there have been no reports of Bayesian analysis being applied to any controlled test marketing experiments in the manner to be suggested in Chapter II. Before focusing on the analysis, the remaining paragraphs of this chapter are used to more clearly deliniate the objective of the experiment that was conducted.

The Experimental Objective

Recognizing the importance of the total merchandising mix to the success of new offerings, an early step in designing the test marketing experiment was to determine what the feasible alternative merchandising strategies were in this situation.

First, the firm had not yet made a final choice of the actual physical product to be sold. The preliminary research and testing that had been conducted had narrowed the list of candidate products to three, all of which management felt were acceptable, provided they would produce adequate profits for the firm and shop manager. From this group, the firm wished to choose one product for chainwide adoption.

The other merchandising variables considered at this time were advertising, packaging, branding, pricing and point-of-purchase promotional material. In determining an appropriate merchandising mix for chocolates, advertising and branding were not variables that could be manipulated. The firm involved follows a family-branding strategy and everything sold in the chain's stores carries the firm's retail brand name. Similarly, there was very little latitude with respect to advertising. The firm's 1971 advertising expenditure was approximately $300,000, or less than one half of one percent of sales. Any advertising the firm does is institutional in nature rather than for specific products. Individual products are promoted only through point-of-purchase materials with the rare exception of media advertising for special, limited time offers.

One area of flexibility for developing a merchandising mix for the product eventually chosen is pricing. Currently, broad spectrums of retail prices exist for groups of boxed chocolate products which are essentially the same physical product. For example, in surveying the competitive situation for this possible new product, prices from $1.50 to $3.50 per pound were observed within the same trading area for a particular solid chocolate item manufactured by one firm but retailed under different brand names. As a consequence of this existing pricing variability, management felt very uncertain as to how customers would react to various prices for boxed chocolates bearing the chain's brand. Although uncertain about customer response, management was able to establish pricing limits. Considering the costs of the various physical products being evaluated, the firm's cost accountant felt that a minimum price below which it was very unlikely the firm could make any profit was $2.00 per pound. This, then, became the floor of the price range management would consider. By considering the firm's strived-for image of good quality at fair value, management set an upper bound on prices it would consider at $2.50 per pound. Any price within these limits would be acceptable, provided it generated adequate profits.

The remaining two areas of flexibility in the merchandising mix are packaging and point-of-purchase promotion. While admitting that these items deserved attention and probably interacted significantly with the other areas of the merchandising mix, management chose not to include these variables in the initial experiment. This decision was based on the added costs and lengthened time horizon that would have been engendered by inclusion of these factors in the experiment. As a result of the above considerations, the objective of the test marketing experiment became that of providing management with information concerning the best price-product combination to be employed in chainwide adoption of boxed chocolates.

Summary

The Research Objective

Traditionally, data from designed experiments have been analyzed via analysis of variance techniques. However, when an experimental program is being employed by management to aid in a decision situation, these traditional methods of analysis are found wanting on two counts. First, they provide no information concerning the economic risk associated with a terminal decision as to which is the best experimental treatment and, second, they fail to provide an adequate criterion for deciding when to stop experimenting and finally choose among the alternatives.

Conceptually, a Bayesian decision theory approach to the analysis of experimental data can overcome the limitations cited above. A necessary step from conceptualization to application is the development of practical methodology. The objective of this research is to extend the methodology of Bayesian differential utility analysis to provide procedures for analyzing data from a factorial experiment with covariates. The utility of the methodology is demonstrated by applying it to test market data that was gathered to help a retail food chain with a product-assortment decision.

The Plan of the Chapters

The remainder of this dissertation is organized in the following way. Chapter II begins with a discussion concerning the choice of an experimental design for the marketing management problem at hand. This discussion is followed by a brief review of some suggested approaches for using experimentation to choose the best from a group of competing courses of action. The differential utility approach to solving this "best process" problem is then discussed in detail and, finally, the differential utility methodology for analyzing data from a factorial design with covariates is developed.

Chapter III is primarily concerned with the data analysis. First, an account is given of the conditions under which the experiment was performed. This account is followed by a presentation of the results of the differential utility analysis. The chapter ends with a comparison between the analysis that was performed and the kind of information that could have been expected from a classical analysis.

Chapter IV concludes the dissertation with a summary of the research, a discussion of its limitations and some suggestions for possible directions of future research.

FOOTNOTES

[1]Robert Schlaifer, Probability and Statistics for Business Decisions (New York: McGraw-Hill, 1959), pp. 486-495.

[2]Robert J. Holloway, "Experimental Work in Marketing: Current Research and New Developments," in Bass, King and Pessemier, eds., Applications of the Sciences in Marketing Management (New York: John Wiley and Sons, Inc., 1968), p. 384.

[3]See M. Cohen and E. Nagel, An Introduction to Logic and Scientific Method (New York: Harcourt, Brace and World, Inc., 1934), pp. 191-196.

[4]See K. Cox and B. Enis, Experimentation for Marketing Decisions (Scranton: International Textbook Co., 1969), p. 101; or R. Frank, in Frank, Kuehn and Massy, eds., Quantitative Techniques in Marketing Analysis (Homewood, Ill.: Richard D. Irwin, Inc., 1962), p. 22.

[5]Schlaifer, p. 615.

[6]Schlaifer, p. 647.

[7]See, for example, Seymour Banks, Experimentation in Marketing (New York: McGraw-Hill, 1965), pp. 236-249; and Cox and Enix, p. 109.

[8]P. Green and D. Tull, Research for Marketing Decisions (Englewood Cliffs, N. J.: Prentice-Hall, Inc., 1970), p. 501.

[9]Schlaifer, pp. 486-495; Howard Raiffa and Robert Schlaifer, Applied Statistical Decision Theory (Cambridge, Mass.: M.I.T. Press, 1968), pp. 139-175.

[10]Philip Kotler, Marketing Management (Englewood Cliffs, N. J.: Prentice-Hall, Inc., 1967), p. 298.

[11]See, for example, D. Hardin, "A New Approach to Test Marketing," Journal of Marketing, 30, No. 4 (October, 1966), pp. 28-31; and Philip Kotler, "Marketing Mix Decisions for New Products," Journal of Marketing Research, 1, No. 1 (February, 1964), pp. 43-49; and Glen Urban, "A New Product Analysis and Decision Model," Management Science, 14, No. 8 (April, 1968), pp. B490-B517, especially pp. B490-B497.

[12]See, for example, Edgar Pessemier, New Product Decisions and Analytical Approach (New York: McGraw-Hill, 1966), pp. 81-140; and Urban, pp. B499-B502.

[13]See, for example, A. Charnes, et al., "DEMON: Decision Mapping via Optimum GO-No Networks--A Model for Marketing New Products," Management Science, 12, No. 11 (July, 1966), pp. 865-888; and Philip Kotler, in Bass, King and Pessemier, "Computer Simulation in the Analysis of New-Product Decisions," pp. 283-331; and Myers and A. Samli, "Management Control of Marketing Research," Journal of Marketing Research, 6, No. 3 (August, 1969), pp. 267-277.

[14]F. Ladik, L. Kent, and P. Nahl, "Test Marketing of New Consumer Pruducts," Journal of Marketing, 24, No. 2 (April, 1960), p. 30.

[15]See T. Floyd and R. Stout, "Measuring Small Changes in a Market Variable," Journal of Marketing Research, 7, No. 1 (February, 1970), pp. 114-116; and D. Hardin, "A New Approach to Test Marketing," and D. Hardin and R. Marquardt, "Increasing Precision of Market Testing," Journal of Marketing Research, 3, No. 4 (November, 1967), pp. 396-399.

[16]See, for example, W. Alderson and P. Green, Planning and Problem Solving in Marketing (Homewood, Ill.: Richard D. Irwin, Inc., 1964), pp. 216-233; and F. Bass, "Marketing Research Expenditures: A Decision Model," Journal of Business (January, 1963), pp. 77-90; and Kotler, "Computer Simulation in the Analysis of New-Product Decisions," pp. 294-297.

[17]See, for example, K. Cox, "The Role of Experimentation in the Information System of a Retailer," in L. G. Smith, ed., Reflections on Progress in Marketing (Chicago: American Marketing Association, 1964), pp. 383-391; and R. Buzzell, W. Salmon, and R. Vancil, Product Profitability Measurement and Merchandising Decisions (Boston: Harvard University Press, 1965), pp. 49-80.

CHAPTER II

THE EXPERIMENTAL DESIGN AND METHODOLOGY FOR ANALYSIS

The Experimental Design

Investigating Interactions

It is widely recognized that in marketing situations, the net effect upon some variable of interest, such as sales, is seldom the sum of a collection of independent effects. There are often substantial interdependencies among the effects of different elements of the marketing mix. The need to use experimental designs that provide for examination of these possible interactions has been articulated by many marketing researchers. For example, Green and Tull point out that being able to study interaction effects ". . . is particularly important in market experimentation where the researcher is typically interested in the combination of controlled variables which leads to the best payoff in terms of sales, cash flow or some other measure of effectiveness."[1]

Factorial designs--designs which allow the simultaneous variation of experimental factors--are the only type of design which provide information pertaining to the interactions of experimental factors.[2] Given this fact and the fact that the object of this research is to provide information on the question of the best price-product combination, a factorial design is mandated for this experiment. Product and price are, of course, the two factors involved. The product factor has three levels--the alternative products being considered. Price also has three levels in the experiment performed, the lowest price that the firm's cost accountant felt a priori could generate profits, the highest price that management felt was consistent with the firm's credo of good value at a fair price, and a level halfway between these extremes.

Controlling Nonexperimental Variables

Certainly price and product are not the only two factors likely to affect boxed chocolate sales. If the results of this test market are to have any external validity,* control over the influence of extraneous variables upon sales must be exercised. Usual methods employed in experimental design for controlling the influence of extraneous variables are random assignment of treatment to test units, inclusion of the extraneous variables in the experimental design, and holding the extraneous variables constant during the experiment.[3]

The principle of randomization to insure statistical equality of test groups is commonly accepted. However, when specific extraneous variables are thought to influence the dependent variable and these extraneous variables can be identified and measured, it is often worthwhile to incorporate them in the experimental design and vary them systematically. This can be a very expensive form of control, though, as the number of test units needed grows rapidly as the number of experimental factors is increased. Further, in marketing experiments, it often is not even possible to incorporate important extraneous variables into an experimental design because the variables are beyond the control of

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*Here the term "external validity" is being used in the same sense as it is by Kerlingher [pp. 301-2] to mean representativeness or generalizeability.

the experimentor. The third tactic for exercising control in an experiment is to attempt to neutralize the effects of extraneous variables by holding them constant throughout the experiment. This tactic is particularly useful when it is thought that there are no interaction effects between the experimental and extraneous variables. If there are interaction effects, they cannot be detected with this type of design. Hence, the power to generalize the results to other levels of the extraneous variables is diminished.

A variation of this third tactic, which is sometimes employed, is to let the extraneous variables vary as they will but to attempt to segregate their effects from the effects of the experimental variables through an "analysis of covariance."[4] Here, again, this strategy is most useful when it is felt there are no interactions between the extraneous and experimental variables.

During the planning stage of this study, a large amount of time was spent discussing the amount and type of control that should be exercised over variables that were not directly concerned with the central questions of this study. It was finally decided that extraneous variables over which the firm could exercise control (for example, promotion and packaging) would be held constant from test unit to test unit and that randomization would be relied on to insure statistical equality of test units with respect to all uncontrollable factors except the two discussed below. While holding the controllable variables constant will highlight the effects of the experimental variables, this strategy runs counter to the logic of the discussion above concerning interaction effects. The fact that other marketing mix variables were not incorporated in the design and varied systematically was due to considerations of the added cost and lengthened time horizon of a larger study.

Randomization was relied upon to insure statistical equality of test units along all uncontrollable dimensions except two, existing sales volume of the test stores and the amount of boxed chocolate sales competition faced by the test stores. It was felt that the effects of these two factors upon sales would be too important to rely on randomization to "average out" their effects. Yet, because neither of these variables is controllable, they could not be directly incorporated into the experimental design and systematically varied. Also, due to the unavailability of enough evenly matched stores, it was not feasible to hold these variables constant across test units. For these reasons it was decided to use an analysis of covariance design to segregate the effects of store size and competition from the effects of price and product upon sales. Attention is now turned to presenting the mathematical model used for the experiment.

The Experimental Model

The model for a 3^2 factorial design is often written

$$y_{ijk} = \mu + \tau_i + \beta_j + \varepsilon_{ijk} \qquad \begin{array}{l} i = 1, 2, 3 \\ j = 1, 2, 3 \\ k = 1, \ldots, n . \end{array} \tag{1}$$

However, it can also be written in terms of a regression model with dummy variables as[5]

$$\underset{(n \times 1)}{Y} = \underset{(n \times 16)}{X} \; \underset{(16 \times 1)}{B} + \underset{(n \times 1)}{E} . \tag{2}$$

The parameters of the model in equation (2) cannot be estimated via regression techniques, however, because the matrix $\underline{X}$ is not of "full rank" and, thus, $(\underline{X}^t\underline{X})$ is singular. In order to use regression techniques, the model must be reparameterized so that the elements of the vector $\underline{B}$ represent relative effects rather than absolute ones.[6] The revised dummy regression model for a 3^2 factorial experiment is

$$\underset{(n\times 1)}{\underline{Y}} = \underset{(n\times 9)}{\underline{X}}\;\underset{(9\times 1)}{\underline{B}} + \underset{(n\times 1)}{\underline{E}}\,. \tag{3}$$

The final step to the appropriate model for this research is to modify equation (3) for consideration of the covariates.

In general, in a situation where it is felt that the mean response for various treatments is influenced not only by the experimental factors but also (independently) by some set of nuisance variables, a model of the form

$$\underline{Y} = [\underline{X}, \underline{W}]\begin{bmatrix}\underline{B}^*\\ \underline{\gamma}\end{bmatrix} + \underline{E}^* \tag{4}$$

is appropriate. Given the assumption that the effects upon the response of the nuisance variables are independent of the effects of the experimental factors, all of the elements of the vectors $\underline{B}^*$ and $\underline{B}$ will be identical with the exception of the first element from each vector. Further, defining $\underline{X}_0$ as the first column of $\underline{X}$, $(\underline{X}_0\,\beta_0 + \underline{E})$ is equal to $([\underline{X}_0, \underline{W}]\,[\beta_0^*, \underline{\gamma}^t]^t + \underline{E}^*)$. If any of the elements of $\underline{\gamma}$ are different from zero, the common variance of the elements of $\underline{E}^*$ will be smaller than that of $\underline{E}$ and using equation (4) will produce more refined estimates of the common β_i than would be obtained using equation (3).[7] Specifically, in the case at hand, $\underline{W}$ is an $(n\times 3)$ matrix with w_1 and

w_2 being dummy variables for moderate and heavy competition and w_3 a quantitative variable for store size as measured by previous sales volume. $\underline{\gamma}$ is correspondingly a (3×1) vector of nuisance parameters to be estimated. The model for this experiment then is

$$\underset{(n \times 1)}{\underline{Y}} = \left[\underset{(n \times 9)}{\underline{X}}, \underset{(n \times 3)}{\underline{W}} \right] \begin{bmatrix} \underset{(9 \times 1)}{\underline{B}} \\ \\ \underset{(3 \times 1)}{\underline{\gamma}} \end{bmatrix} + \underset{(n \times 1)}{\underline{E}} . \tag{5}$$

Note again that the effects of the covariates are assumed to be independent of the effects of the experimental factors. If such is not the case, then the β_i of equation (3) are not equivalent to the β_i of equation (5) and equation (5) is not appropriate. However, in this particular situation, there appears to be no reasons, a priori, to feel that the assumption of independence of effects is untenable.

Methodology for Analysis

The Traditional Methodology

The usual procedure for analyzing data generated according to the above model would be to conduct analysis-of-variance "F" tests to test the importance of the experimental factors and the differences among the average responses of the various treatments--or perhaps more appropriately to use some method such as Tukey's test,[8] or Scheffé's procedure[9] to isolate the treatment(s) whose mean response(s) is/are significantly greater than those of the other treatments. All of these procedures, however, have serious shortcomings when the results of the analysis must be used in a decision context. Even when the criterion variable is a proper measure of utility, tests of this nature are insufficient because they provide information only on the probability of an incorrect choice. They provide no measure of the economic risk involved in choosing one treatment over the others nor do they provide sufficient guidelines for determining whether or not further testing is appropriate prior to making the final choice among competing alternatives.

Suggested Improvements in Methodology

Many authors have addressed themselves to these shortcomings of the traditional methodology for choosing the best from among a group of competing treatments. Two questions must be answered in coping with the "best process problem": (1) How should the best process be chosen? and (2) In order to reduce the risk of an incorrect decision, how much information should be gathered before a final decision is made? Historically, several avenues of attack have been suggested for improving upon the classical approach to this problem. Bechhofer,[10] for example, has suggested a procedure which is essentially to take a sample of size n from each of the competing processes, compute the sample means with respect to the utility criterion and choose the process with the largest sample mean. In Bechhofer's procedure, however, the sample size, n, is chosen in a slightly different fashion than is customary in analysis of variance. To determine n, the experimentor must specify two things: the smallest difference between the largest and next largest process means that is economically worth detecting and the conditional probability of a correct choice among the processes, given that the difference between the best and second best processes is exactly the critical size specified. While perhaps representing some improvement over the classical methods, Bechhofer's method is still far from complete as it does not incorporate any formal consideration of the consequences of a wrong decision and it restricts itself to making a decision based solely on the results of a single sample.

A more comprehensive procedure in that it includes considerations of the consequences of wrong decisions has been suggested by Somerville.[11] Somerville's procedure is also to choose among the treatments on the basis of the results of a single sample. However, in determining sample size Somerville introduces a linear loss function and, before experimenting, computes the conditional expected loss of a wrong decision, conditioned upon the unknown values of the process means. This conditional expected loss is a function of the sample size. To determine the appropriate sample size, Somerville combines the expected loss information with information on the cost of sampling and employs a minimax decision rule.

Grundy, Healy and Rees have suggested still a different approach.[12] These authors assume that there is prior information available concerning the mean utilities of the treatments. This information is assumed to have come from a preliminary experiment. A linear loss function is assumed and integrated with respect to the prior distribution. The optimal experimental size is then determined by choosing n to minimize the expected loss. Equivalent mathematical results could have been derived by assuming an a priori subjective distribution of the unknown treatment means.[13] These authors dealt only with the special case of two competing processes. Dunnett has generalized their results to deal with choice problems involving several processes.[14] Dunnett's assumption concerning the prior information was that this information was in the form of a multivariate normal distribution for the unknown treatment means. He also assumed that the mean vector and covariance matrix for this a priori distribution were both known.

Differential Utility Analysis

In tackling the best process problem, Schlaifer has also suggested a procedure for choosing between two processes which incorporates prior information as well as current sample information and which chooses on the basis of expected economic consequences. However, the methodology is simplified somewhat by the introduction of the concept of "differential utility."[15] The multivariate extension of the differential utility concept is given by Raiffa and Schlaifer.[16] Briefly, these authors point out that in many choice situations, the decision depends not upon the absolute utilities associated with the alternatives but rather upon their relative utilities. Defining $\underline{\tilde{\Delta}}$, the vector of unknown differences in utility between the optimal and various nonoptimal treatments reduces the analysis of a problem to a series of systematic transformations of probability distributions followed by taking the expectation of a fairly "clean" loss function with respect to the transformed probability distribution.[17]

As with all the other authors so far mentioned, Raiffa and Schlaifer deal only with completely randomized experimental designs. Schlaifer, however, points out that the amount of information to be gained from n experimental observations is influenced in large part by the design of the experiment.[18] This adds a new dimension of complication to the problem of determining the optimal experimental strategy and, to this writer's knowledge, no one has developed the decision theory methodology necessary to specify both the design and size of the optimal experiment for the general best process problem.

Expanding upon the work of Raiffa and Schlaifer, Frederick has pointed out that the differential utility concept can be used to evaluate potential experiments with other than randomized designs and he has developed the necessary methodology for several experimental designs commonly used in marketing.[19] While it is not possible to identify the global optimal experiment with the methods of Raiffa and Schlaifer and Frederick, it is possible to rank various experiments that are under consideration on the basis of expected net gain of sampling and, thus, to determine which one of them, if any, is worth performing prior to making a final choice among treatments. In the paragraphs below, the work of Raiffa and Schlaifer and Frederick is reviewed and extended to develop the differential utility methodology for analyzing experimental data from a factorial design with covariates. In the next chapter, this methodology is applied to the data generated in this research study.

In developing their methodology for the best process problem, Raiffa and Schlaifer make three assumptions: (1) that the utilities of terminal action and experimentation are additive, (2) that the terminal utility of adopting any particular treatment is linear in the "quality" of that treatment and independent of the qualities of the other treatments and (3) that sample observations from the ith process are normally and independently distributed with mean μ_i.[20] These are the same assumptions to be used here.

The analysis begins with the $(p \times 1)$ vector $\underline{\tilde{\mu}}$ of "qualities" of the competing treatments. The tilde indicates that the elements of the vector are not known with certainty but rather have some probability distribution associated with them. According to assumption (2), there exists a linear transformation which maps $\underline{\tilde{\mu}}$ into utility space. Accordingly, the equation

$$\underline{\tilde{U}} = \underline{C} + \underline{K\tilde{\mu}} \tag{6}$$

defines the $(p \times 1)$ random vector of utilities associated with the p treatments. When an estimate of $\underline{\tilde{\mu}}$ is available (in terms of a probability distribution), terminal analysis is simply a matter of transforming the distribution of $\underline{\tilde{\mu}}$ to that of $\underline{\tilde{U}}$, finding the expected value of $\underline{\tilde{U}}$ and choosing the treatment associated with the maximum of $E(\tilde{u}_1, \tilde{u}_2, \ldots, \tilde{u}_p)$. Two questions remain: (1) What is the economic risk of making a terminal decision based on the information at hand? and (2) Is it worthwhile to experiment further before making a terminal decision? These questions can be answered via differential utility analysis.

Without loss of generality, the elements of $\underline{\tilde{U}}$ can be numbered in such a way that the pth element of $\underline{\tilde{U}}$ is associated with the treatment that is optimal in terms of the current information state. From this, the $(p-1 \times 1)$ vector $\underline{\tilde{\Delta}}$ is defined as

$$\underline{\tilde{\Delta}} = \underline{A\tilde{U}} \tag{7}$$

where $\underline{A}$ is constructed so that the $(p-1)$ elements of $\underline{\tilde{\Delta}}$ represent

$(\tilde{u}_i - \tilde{u}_p)$ for $i < p$; that is, the differential utilities between the optimal and the nonoptimal treatments.

In defining $\underline{\tilde{\Delta}}$, the elements of $\underline{\tilde{U}}$ were ordered such that $\tilde{u}_p$ had the largest <u>expected</u> value. However, the <u>actual</u> value of $\tilde{u}_p$ may be exceeded by the $\tilde{u}_i$ associated with one or more of the other treatments. If perfect information regarding $\underline{U}$ were available, the decision maker would choose the jth treatment, where u_j was the largest element of $\underline{U}$, rather than choose the pth treatment, the one that is optimal given the present information state. The value of this perfect information would be

$$\begin{aligned} \text{VPI} &= \max\,(u_1, u_2, \ldots, u_p) - u_p \\ &= \max\,(u_1 - u_p), (u_2 - u_p), \ldots, (u_p - u_p) \end{aligned} \tag{8a}$$

or, in terms of differential utilities,

$$\text{VPI} = \max\,(\delta_1, \delta_2, \ldots, \delta_{p-1}, 0)\,. \tag{8b}$$

Before perfect information is actually received, its value is a random variable. The expected value of perfect information can be computed by first transforming the distribution of $\underline{\tilde{U}}$ to that of $\underline{\tilde{\Delta}}$ and then taking the expected value of this transformed distribution

$$\text{EVPI} = E\,\{\max\,(\tilde{\delta}_1, \tilde{\delta}_2, \ldots, \tilde{\delta}_{p-1}, 0)\}\,. \tag{9}$$

The expected value of perfect information is also the expected loss associated with the decision and, thus, serves as a measure of the risk to the decision maker of making a final choice among treatments based on the information at hand.

Whether or not it is worthwhile to gather more information depends upon whether or not the added information can reduce the expected loss associated with the decision by an amount greater than the cost of the additional information; that is, on whether or not the expected value of sample information is greater than its cost.

After an experiment is performed, the distribution of $\underline{\tilde{\mu}}$ and, hence, the distribution of $\underline{\tilde{U}}$ will be revised. The decision maker will now choose the treatment that is optimal under this updated or posterior distribution. The resulting increase in utility or the value of this information is

$$\begin{aligned} \text{VSI} &= \max \text{E} \, (\tilde{u}''_1, \ldots, \tilde{u}''_p) - \text{E}(\tilde{u}'') \\ &= \max \text{E} \, \{(\tilde{u}''_1 - \tilde{u}''_p), \ldots, (\tilde{u}''_p - \tilde{u}''_p)\} \\ &= \max \text{E} \, (\tilde{\delta}''_1, \ldots, \tilde{\delta}''_{p-1}, 0) \\ &= \max \, (\bar{\delta}''_1, \ldots, \bar{\delta}''_{p-1}, 0) \end{aligned} \tag{10}$$

where the double prime indicates that the expectation is with respect to the posterior distribution and $\bar{\delta}''_i = \text{E}(\tilde{\delta}''_i)$. Before the experiment is actually performed, however, the experimental results are unknown and, thus, the various $\tilde{\bar{\delta}}''_i$ are random variables. Because of this uncertainty, the decision of whether or not to conduct a given experiment must be based on the expected value of sample information for that experiment. The expected value of sample information yields the net gain of sampling and provides an economic criterion for deciding whether or not it is worth conducting the additional experimentation. If there is more than one candidate experiment, the decision maker can, of course, choose among them on the basis of their respective net gains of sampling.

If some experiment is in fact chosen and performed, the decision maker is not then necessarily committed to terminal action. The distribututtion $\tilde{\underline{\mu}}''$ can be redesignated as the prior distribution $\tilde{\underline{\mu}}'$ with regard to further potential experiments and the procedures outlined above may be repeated. This iterative procedure can continue until it appears that no candidate experiment is economically justified. At that point, a final choice among alternatives should be made.

It remains to develop in detail the various distributions of $\tilde{\underline{\mu}}$, $\tilde{\underline{U}}$ and $\tilde{\underline{\Delta}}$ that are needed to conduct the analysis outlined above. By assumption (3), the data-generating process involved is multivariate normal. The conjugate family of distributions of $\tilde{\underline{\mu}}$ then is multivariate normal.*[21] From this, the distributions of $\tilde{\underline{U}}$ and $\tilde{\underline{\Delta}}$ follow quite neatly.

Denote the mean and variance of $\tilde{\underline{\mu}}$ by $\bar{\underline{\mu}}$ and $\underline{\Sigma}_{\mu}$, respectively. By equation (6), $\tilde{\underline{U}}$ is a linear transformation of $\tilde{\underline{\mu}}$ and, thus, $\tilde{\underline{U}}$ is also multivariate normal with parameters[22]

$$\bar{\underline{U}} = E(\tilde{U}) = E(\underline{C} + \underline{K\tilde{\mu}}) = \underline{C} + \underline{K\mu} \tag{12a}$$

and

$$\underline{\Sigma}_{\mu} = V(\tilde{U}) = V(\underline{C} + \underline{K\tilde{\mu}}) = \underline{K\Sigma_{\mu}K}^{t} . \tag{12b}$$

Since $\tilde{\underline{\Delta}}$ is in turn a linear transformation of $\tilde{\underline{U}}$ (equation (7)), $\tilde{\underline{\Delta}}$ is in turn multivariate normal with parameters

$$\bar{\underline{\Delta}} = E(\tilde{\underline{\Delta}}) = E(\underline{A\tilde{U}}) = \underline{AC} + \underline{AK\bar{\mu}} \tag{13a}$$

and

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*Strictly speaking, the distribution of $\tilde{\underline{\mu}}$ will be multivariate normal if the covariance matrix for the processes is known. When this covariance matrix is unknown, the marginal distribution of $\tilde{\underline{\mu}}$ is multivariate Student t (See Raiffa and Schlaifer, p. 320). In what follows, it is assumed that the degrees of freedom in the initial experiment were large enough to warrant treating the estimate of σ^2, the common variance of the error terms in equation (5), as a certainty equivalent and, thus, that the distribution of $\tilde{\underline{\mu}}$ is normal.

$$\underline{\Sigma}_{\delta} = V(\underline{\tilde{\Delta}}) = V(\underline{A}\underline{\tilde{U}}) = \underline{A}\underline{K}\underline{\Sigma}_{\mu}\underline{K}^{t}\underline{A}^{t} . \tag{13b}$$

Equations (12) and (13) outline the fundamental relationships among the distributions of $\underline{\tilde{\mu}}$, $\underline{\tilde{U}}$ and $\underline{\tilde{\Delta}}$. The procedure for updating the distribution of $\underline{\tilde{\mu}}$ (and, thus, the distributions of $\underline{\tilde{U}}$ and $\underline{\tilde{\Delta}}$) in light of new experimental evidence follows.

Prior to the collection of any additional information $\underline{\bar{\mu}}'$ and $\underline{\Sigma}'_{\mu}$ summarize what is known regarding the distribution of $\underline{\tilde{\mu}}$. When an experiment yields new information summarized by the sufficient statistics $\underline{\bar{\mu}}$ and $\underline{\Sigma}_{\mu}$, this sample information is combined with the prior information to yield posterior parameters of the distribution of $\underline{\tilde{\mu}}$. The relationships between the prior parameters, the sample statistics and the posterior parameters, are[23]

$$\underline{\bar{\mu}}'' = [\underline{\Sigma}'^{-1}_{\mu} + \underline{\Sigma}^{-1}_{\mu}]^{-1}[\underline{\Sigma}'^{-1}_{\mu}\,\underline{\bar{\mu}}' + \underline{\Sigma}^{-1}_{\mu}\underline{\bar{\mu}}] \tag{14a}$$

and

$$\underline{\Sigma}''_{\mu} = [\underline{\Sigma}'^{-1}_{\mu} + \underline{\Sigma}^{-1}_{\mu}]^{-1} \tag{14b}$$

where double primes denote the posterior parameters.

After an experiment has been performed, then the posterior distribution of $\underline{\tilde{\mu}}$ can be transformed to the posterior distribution of $\underline{\tilde{\Delta}}$ and the value of sample information can be computed according to equation (10). This after-the-fact calculation is of little use, however, in trying to decide before the fact whether or not the experiment should be conducted. Instead, the decision maker must compute the expected value of sample information before the sample is actually taken; that is, when the posterior parameter $\underline{\overline{\mu}}''$ is itself a random variable.* The before-sampling or preposterior distribution of $\underline{\tilde{\overline{\mu}}}''$ is multivariate normal with parameters[24]

$$\underline{\overline{\mu}}' = E(\underline{\tilde{\overline{\mu}}}'') \tag{15a}$$

and

$$\underline{\Sigma}^{*}_{\mu} = V(\underline{\tilde{\overline{\mu}}}'') = \underline{\Sigma}'_{\mu} - \underline{\Sigma}''_{\mu} . \tag{15b}$$

This outlines the procedure for updating the distribution of $\underline{\tilde{\mu}}$. The updated distributions of $\underline{\tilde{U}}$ and $\underline{\tilde{\Delta}}$ are obtained by applying equations (12) and (13) to the appropriate distribution of $\underline{\tilde{\mu}}$.

Differential Utility Analysis for a Factorial Design with Covariates

With the relationships among the distributions of $\underline{\tilde{U}}$ and $\underline{\tilde{\Delta}}$ and the procedures for updating these distributions laid out, all that remains is to specify the exact form of $\underline{\overline{\mu}}$ and $\underline{\Sigma}_{\mu}$ and explain the procedure for evaluating the expectations in equations (9) and (11).

Refer to equation (3), the model for a 3^2 factorial experiment. The mean response for the ith experimental treatment, y_i $(i = 1, \ldots, 9)$, is

$$\overline{y}_i = \underline{x}_i \underline{B} \tag{16}$$

where $\underline{B}$ is the (unknown) vector of coefficients in (3) and $\underline{x}_i$ is the appropriate (1×9) vector of dummy variables. $\overline{y}_i$ is the mean quality of the ith treatment. From here on, this quantity will be called μ_i to conform with the notation used above.

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*Although $\underline{\overline{\mu}}''$ is not known with certainty before experimentation, $\underline{\Sigma}^{*}_{\mu}$ will be a certain quantity, provided again that the covariance matrix of the data-generating process is known.

When covariates are added to the 3^2 factorial model as in equation (5), the relationship of equation (16) is modified to

$$\mu_i = [\underline{x}_i, \underline{w}_i] \begin{bmatrix} \underline{B} \\ \hline \underline{\gamma} \end{bmatrix} \tag{17}$$

and the value of μ_i is dependent upon the levels of the covariates; that is, upon the values of the elements of $\underline{w}_i$. Since interest in this study centers on the effects of the treatment variables, in what follows, the levels of the covariates are held constant at their means. Thus, the μ_i should be viewed as the average response to the ith treatment for an average size store facing moderate competition.

If $\underline{X}_\tau$ is defined as the (9×12) "treatment matrix," each row of which is one of the $[\underline{x}_i, \underline{w}_i]$; and if $\begin{bmatrix} \underline{B} \\ \hline \underline{\gamma} \end{bmatrix}$ is redefined as $\underline{B}$ for notational ease, the vector of mean responses, $\underline{\mu}$, is

$$\underset{(9\times 1)}{\underline{\mu}} = \underset{(9\times 12)}{\underline{X}} \; \underset{(12\times 1)}{\underline{B}} \,. \tag{18}$$

The elements of $\underline{B}$ are, of course, unknown. Under the usual assumptions of normal regression theory,[25] the Gauss-Markov theorem is applicable and $\underline{B}$ may be estimated from data by[26]

$$\underline{\tilde{B}} = \left[[\underline{X},\underline{W}]^t \, [\underline{X},\underline{W}] \right]^{-1} [\underline{X},\underline{W}]^t \underline{Y} \,. \tag{19}$$

$\underline{\tilde{B}}$ will be multivariate normal with parameters

$$\underline{B} = E(\underline{\tilde{B}}) \tag{20a}$$

and

$$\underline{\Sigma}_b = V(\underline{\tilde{B}}) = \sigma^2 \left[[\underline{X},\underline{W}]^t \, [\underline{X},\underline{W}] \right]^{-1} \tag{20b}$$

where σ^2 is the common variance of the error terms in equation (5). Since $\underline{\tilde{\mu}}$ is a linear transformation of $\underline{\tilde{B}}$ (equation (18)), the same transformation arguments used above apply and, consequently, $\underline{\tilde{\mu}}$ will be multivariate normal with parameters

$$\underline{\overline{\mu}} = E(\underline{\tilde{\mu}}) = E(\underline{X}_{\tau}\underline{\tilde{B}}) = \underline{X}_{\tau}\underline{B} \tag{21a}$$

and

$$\underline{\Sigma}_{\mu} = V(\underline{\tilde{\mu}}) = V(\underline{X}_{\tau}\underline{\tilde{B}}) = \underline{X}_{\tau}\Sigma_{b}\underline{X}_{\tau}^{t} . \tag{21b}$$

One final detail remains. The actual computation of both the expected value of perfect information and the expected value of sample information (equations (9) and (11)) involve the evaluation of multivariate normal linear loss integrals. These integrals can be evaluated using Monte Carlo methods.[27] The steps involved in such an evaluation are outlined here. First, a $(p-1 \times 1)$ vector $\underline{\tilde{Z}}$ is constructed by drawing from the unit spherical normal distribution. $\underline{\tilde{Z}}$ is transformed to a drawing from the distribution of $\underline{\tilde{\Delta}}$ by using the relationship

$$\underline{\tilde{\Delta}} = \underline{\overline{\Delta}} + \underline{S}^{-1}\underline{\tilde{Z}} \tag{22}$$

where $\underline{\overline{\Delta}}$ is the mean vector of the distribution of $\underline{\tilde{\Delta}}$ and $\underline{S}$ is defined by

$$\underline{S}\underline{\Sigma}_{\delta}\underline{S}^{t} = \underline{I} . \tag{23}$$

The conditional loss for this draw is

$$l = \max (\tilde{\delta}_{1}, \ldots, \tilde{\delta}_{p-1}, 0) \tag{24}$$

and the expected loss based on n draws is

$$EL = \overline{l} = 1/n \sum_{i=1}^{n} (l_{i}) . \tag{25}$$

The Monte Carlo procedure is terminated when the variance of $\overline{l}$ is "small." When $\underline{\Sigma}'_{\delta}$ is used to define $\underline{S}$, the result of the computation in equation (25) is the expected value of perfect information. When $\underline{\Sigma}''_{\delta}$ is used, the computation yields the expected value of sample information.

The methodology for analyzing data generated according to equation (5) within a decision framework is now developed. To review briefly, the analysis is conducted by estimating $\underline{B}$ via a regression analysis of experimental data and then transforming $\underline{\tilde{B}}$ to $\underline{\tilde{\mu}}$ and in turn to $\underline{\tilde{U}}$. The optimal treatment can then be identified. To determine the economic risk involved in making a choice among the alternatives with this information, the distribution of $\underline{\tilde{U}}$ is further transformed to that of $\underline{\tilde{\Delta}}$ and the expected value of perfect information is calculated. Finally, to decide whether or not further testing is warranted, the net gains of sampling for potential experiments are computed.

Summary

Interdependencies among effects due to different elements of the marketing mix dictated a factorial design for investigating the effects of price and product upon profits from candy sales. To heighten external valifity, the basic factorial design was modified to include consideration of two covariates which were beyond the direct manipulative control of the firm--store size and candy competition. A traditional analysis of the data generated by this experiment would have fallen short on two counts; there would not have been an adequate measure of the risk associated with a terminal decision and there would not have been an adequate criterion for deciding when to terminate experimentation. These two shortcomings can be overcome by utilizing the decision theoretic methodology that was developed in this chapter. In the next chapter this methodology is applied to the data of this study.

FOOTNOTES

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CHAPTER III

DATA COLLECTION AND ANALYSIS

The Experimental Conditions

The Test Units

Because the purpose of this experiment was to provide information regarding the relative profitability of the various price-product combinations under normal operating conditions, every effort was made in the experiment to duplicate the conditions the products would face should they be adopted chainwide. This meant that simultaneous testing of all the treatments in the 3^2 factorial experiment would require at least nine test stores so that no two treatments would appear in the same store. Testing the different treatments side by side in the same store would have introduced a comparison situation which, under normal conditions, would not exist since only one product is to be adopted. Testing a single treatment per store provides conditions most similar to normal circumstances. Following the guidelines of Ladik, Kent and Nahl, each treatment was placed in two geographically separated stores rather than in a single store.[1] The total of 18 stores involved in the experiment represent approximately 6 percent of the total stores in the chain.

The test stores were located in six geographically separate markets, three stores to a market. The geographic separation was thought to be needed to insure representativeness of the sample and, thus, enhance generalizeability and also to guard against contamination of the data caused by having different prices appear in the same market area.

While the market areas were chosen to insure geographic separation, the test stores within each area were chosen randomly from the group of shops in that area. One store initially selected in the random draw was discarded at management's request because of both intraorganizational and customer relations problems peculiar to that store at the time of the experiment. The discarded store's place was taken by another randomly selected store from the same market.

The experimental treatments were assigned to stores as follows. First, two market areas were randomly assigned to each of the price levels ($1.99, $2.25, and $2.49). Next, within each market area, the three test products were randomly assigned, one to each test store in the market. The resulting configuration of experimental treatments appears in Table 1 on page 42.

Controlling Extraneous Variables

Marketing mix factors

As discussed in Chapter II, all important extraneous factors within the control of the firm were kept as constant as possible from store to store. Particular attention was paid to the following factors.

Display

The boxed chocolates were afforded the same amount of display space in the same relative position (near the cash register) in each store. A closed box of chocolates and one with a transparent cellophane wrap in

TABLE 1

CONFIGURATION OF EXPERIMENTAL TREATMENTS

Market Area	Price	Store	Product	Competition	Average Sales (Scaled)	# Obs. 9 Wks.	# Obs. 12 Wks.
1	$1.99	a_1	A	Low	1.56	5	7
		b_1	B	Low	1.35	9	12
		c_1	C	Low	.61	7	10
2	$1.99	a_2	A	Low	1.75	9	10
		b_2	B	High	3.47	7	10
		c_2	C	High	2.14	9	12
3	$2.25	a_3	A	Moderate	1.57	8	11
		b_3	B	Low	2.05	7	10
		c_3	C	Low	2.22	9	12
4	$2.25	a_4	A	Low	2.43	8	11
		b_4	B	High	1.82	9	12
		c_4	C	Low	1.98	1	1
5	$2.49	a_5	A	Moderate	2.02	8	11
		b_5	B	Moderate	1.13	9	12
		c_5	C	Low	1.31	5	7
6	$2.49	a_6	A	Moderate	1.84	9	12
		b_6	B	High	2.44	7	10
		c_6	C	High	1.83	9	10
Total						135	180

place of the cover were displayed. Candy to fill customer orders was stored beneath the counter near the display. This display pattern, dictated in large part by the space limitations in existing stores, was essentially the one that would be used, at least initially, for the product adopted chainwide.

Packaging

All candy was sold in rectangular one-pound boxes containing one layer of candy. The boxes for all three products carried the firm's brand name and were essentially the same design. Content labeling was different for each product. Between this descriptive label, the open-faced display box and the point-of-purchase material present, it was felt that the customer could clearly tell the type of chocolate product offered for sale.

Point-of-purchase promotion

Point-of-purchase materials were placed in all the test stores. A counter card (approximately 4 × 5 1/2 inches) was placed at each booth and at each condiment station in stool areas. Easel cards (approximately 9 × 12 inches) were placed at the candy display and at various other locations throughout the store. Daily checks were made to replace cards which disappeared or became soiled, torn or otherwise unserviceable. All the point-of-purchase materials were similar in appearance and style. The cards were printed in the chain's colors and contained a short, conservative, informational message inviting customers to try the boxed chocolates. No artwork or photographs appeared on the cards. The message contained the product name (all three products were given the same name), a brief description of the contents of a box and the price of a box. The message information pertaining to price and physical product was, of course, specifically tailored to each of the nine treatments. No specific claims were made for the products other than that they were in keeping with the firm's traditions of high quality. Materials similar in type to those outlined here are used periodically by the firm for promotion of such things as special ice cream flavors, particular sandwiches and special prices.

Personal selling

In the process of acquainting participating store managers with the test program and the role they would play, an effort was made to communicate the importance of having employees treat this product just as they would any of the firm's established products. Specifically, sales people were to be discouraged from exerting extra selling efforts for the product simply because it was new. It was also emphasized that in this experimental program, the success, or lack thereof, of the product in any store would not reflect favorably or unfavorably upon that store's manager or personnel. Finally, the firm agreed to buy back any unsold stock at the end of the test so that no attempt would be made to reduce inventories of slow-moving merchandise by altering the test conditions.

Environmental factors

Prior to beginning data collection, the values of the covariates for each test store were determined. Store size was measured by averaging the store's monthly sales figures for the immediately preceding twelve months. To measure competition, each store's immediate marketing area was surveyed by the store manager and the experimenter. The amount of competition was qualitatively determined by considering such factors as the number of competitors selling chocolates, types of candy being sold, amount of display space devoted to boxed chocolates or close substitutes, prices, and distances from the test store. Competition was ranked as being low, moderate or high. The covariate values for test stores appear in Table 1.

The Pretest

After the covariates were measured, a pretest was conducted during the middle of August, 1972. During this period, the candy was displayed and sold in the test stores as it would be during the actual test. The pretest was conducted for the following reasons: (1) to allow sufficient time for the novelty aspect of boxed chocolates, and any attendant peculiar buying behavior, to diminish to an insignificant level, (2) to test data collection procedures, (3) to check logistics, (4) to acquaint the personnel of participating stores with the experiment and (5) to insure that the experimental conditions for each store were properly understood and maintained. Minor problems which arose during the pretest were corrected and data for analysis began to be collected the week ending September 9, 1972.

The Data Analysis

Some Preliminaries

A review of the methodology

The data collected in this experiment were subjected to the analysis of Chapter II. To review, the steps in the analysis can be summarized as follows.

1. Estimating the parameters of the regression equation for the experimental design (Chapter II, equation (5)).
2. Estimating the treatment means. Using the information from step (1), an estimate of the average number of boxes of candy which will be sold per week in an average store is made for each of the nine experimental treatments (Chapter II, equation (18)).
3. Deriving the utility and differential utility vectors. Equations (12) and (13) of Chapter II are used to successively transform the data to utility and then differential utility space. Examination of the utility vector reveals the treatment which is optimal under the present information state.
4. Determining the economic risk of a terminal decision with present information. The computation of the EVPI (Chapter II, equation (9)) is performed using the procedures outlined on page 36 of Chapter II. The EVPI is also the expected loss associated with a terminal decision and, thus, serves as a measure of economic risk.

5. Determining the expected value of sample information. $\underline{\Sigma}'_\delta$ is transformed to $\underline{\Sigma}''_\delta$ according to the method on pages 33-34 of Chapter II, and the EVSI (Chapter II, equation (11)) is found by again using the procedure of page 36. And finally,

6. Deciding whether or not to terminate experimentation. If the cost of experimentation exceeds the EVSI, a terminal decision should be made at this point. If, however, the net gain of sampling is positive, the decision should be postponed until the additional data are collected and steps (1) through (5) are repeated.

Carrying out the analysis outlined here involves a large computational burden. In the next section, the two computer programs that were employed in the analysis are discussed.

The computer programs employed

The SAS regression procedure

To perform the first step in the analysis (estimate the regression equation), the regression procedure from the Statistical Analysis System (SAS) was used.[2] The inputs to this program were the reparameterized regression model for this experimental design (Chapter II, equation (5)) and the collected experimental data. The outputs obtained from this program included the vector of estimated regression coefficients, $\hat{\underline{B}}$, the estimated residual error variance, $\hat{\sigma}^2$, and the inverse cross-products matrix, $[[\underline{X},\underline{W}]^t\,[\underline{X},\underline{W}]]^{-1}$. The last two quantities were combined according to equation (20b) of Chapter II to produce the estimated covariance matrix of $\hat{\underline{B}}$, $\hat{\underline{\Sigma}}_b$. Highlights of the SAS output are presented in tables later in this chapter. Further discussion of the SAS package and copies of the complete outputs for the two regression runs discussed below are contained in the Appendix.

Program THESIS

Steps (2) through (5) of the analysis, as outlined above, were performed with the aid of a time-sharing program, entitled THESIS. THESIS was written by this author expressly for the purposes of this research. A complete explanation of THESIS, including flowcharts and a program listing, appears in the Appendix. Here the necessary inputs to THESIS and the outputs of interest are discussed.

The data inputs to THESIS consist of the following: (1) the treatment matrix, $\underline{X}_\tau$ (Chapter II, equation (18)), (2) the utility transformation matrix, $\underline{K}$ (Chapter II, equation (6)), (3) the vector of estimated regression coefficients, $\hat{\underline{B}}$, (4) the estimated covariance matrix of $\hat{\underline{B}}$, $\hat{\underline{\Sigma}}_b$, (5) the estimated residual error variance for the regression, $\hat{\sigma}^2$, and (6) the design matrix for a week's replication of the 18 observation design.

Inputs (3), (4) and (5) are data dependent and must be updated each time new data are introduced to the analysis. These data-dependent inputs were obtained as the outputs from the SAS regression runs described above. The remaining inputs to THESIS are independent of the experimental data and, thus, remain constant from analysis to analysis. These inputs are described below.

The treatment matrix, input (1), consists of nine rows. The ith row of this matrix is the vector by which $\underline{\hat{B}}$ must be premultiplied to arrive at the estimated mean weekly sales for an "average" store under treatment i, μ_i . To define an average store, the two covariates are set at their means. For the qualitative covariate competition, this means setting the dummy variable for moderate competition of "1" and the dummy variable for heavy competition at "0." The (scaled) mean value for the covariate "store size" is 1.81. Table 2 contains the complete treatment matrix for this experimental design.

TABLE 2

TREATMENT MATRIX, $\underline{X}_\tau$

Trmt.	Prod.	Price	Int.	X2	X3	X4	X5	X6	X7	X8	X9	X10	X11	X12
1	A	\$1.99	1	0	0	0	0	0	0	0	0	1	0	1.81
2	A	2.25	1	1	0	0	0	0	0	0	0	1	0	1.81
3	A	2.49	1	0	1	0	0	0	0	0	0	1	0	1.81
4	B	1.99	1	0	0	1	0	0	0	0	0	1	0	1.81
5	B	2.25	1	1	0	1	0	1	0	0	0	1	0	1.81
6	B	2.49	1	0	1	1	0	0	0	1	0	1	0	1.81
7	C	1.99	1	0	0	0	1	0	0	0	0	1	0	1.81
8	C	2.25	1	1	0	0	1	0	1	0	0	1	0	1.81
9	C	2.49	1	0	1	0	1	0	0	0	1	1	0	1.81

Input (2), the utility transformation matrix, was developed in the following fashion. First, for each treatment, the utility to the firm of a store's selling one box of chocolates was determined. These figures were then multiplied by 317, the number of shops in the chain. The resulting figures became the diagonal elements of the (diagonal) utility transformation matrix.

As noted in Chapter I, the firm's goal in seeking new products is not only to increase the chain's profitability but also to increase the compensation of store managers. Each shop in the chain purchases its supplies and items for resale from the parent firm. Retail prices together with some other rather broad operating constraints are determined by the parent firm. Within these constraints, the store manager has a free hand in running the store. The firm's income from the store is in two forms, the wholesale markup on goods and supplies sold to the store and store "rent," which is a percentage of gross store sales. The manager's compensation is the net profit of the store after all bills, including store rent, are paid. After discussions with the firm's management, it was decided that a proper measure of the utility to the firm of selling a box of chocolates was the retail purchase price less the cost of the candy to the firm and the added variable costs to the firm and store manager of handling the candy. The fixed costs involved were very minimal and, hence, were ignored. This utility measure reflects the net gain to the system (the firm and store manager combined) of selling a box of chocolates. Making these calculations for each of the nine treatments and multiplying the resultant figures by 317 gives the values in Table 3 below. It should be noted that ignoring the negligible fixed costs for this particular application means that the matrix $\underline{C}$ of equation (6), Chapter II, is a zero matrix. Thus, the complete utility transformation for this application is obtained by premultiplying $\underline{\hat{\mu}}$ by $\underline{K}$.

TABLE 3

DIAGONAL ELEMENTS OF THE UTILITY TRANSFORMATION MATRIX , K*

Price	Product		
	A	B	C
\$1.99	\$224.436 (k_{11})	\$208.586 (k_{44})	\$208.586 (k_{77})
2.25	306.856 (k_{22})	291.006 (k_{55})	291.006 (k_{88})
2.49	382.936 (k_{33})	367.086 (k_{66})	367.086 (k_{99})

*The off-diagonal elements of K are all zero.

Finally, input (6), the design matrix, contains one row corresponding to each of the 18 stores' experimental conditions, as outlined in Table 1 of this chapter. Table 4 illustrates the design matrix.

THESIS provides as outputs $\hat{\underline{\mu}}$, $\hat{\underline{u}}$, $\hat{\underline{\Delta}}$, the expected value of perfect information, and the expected value of sample information--the quantities necessary for carrying out the differential utility analysis of the experimental data. In addition to these quantities, various intermediate outputs which serve as computational checks are provided by the program. Tables later in the chapter highlight the output of THESIS. Complete details of the entire output from this program appear in the Appendix.

Interpreting the regression equation

The final preliminary before focusing on the analysis proper is to review the regression model for this experimental design and interpret the meanings of the elements of the model's coefficient vector.

TABLE 4

DESIGN MATRIX

Store	Int.	X2	X3	X4	X5	X6	X7	X8	X9	X10	X11	X12
a_1	1	0	0	0	0	0	0	0	0	0	0	1.56
b_1	1	0	0	0	0	0	0	0	0	0	0	1.35
c_1	1	0	0	1	1	0	0	0	0	0	0	0.61
a_2	1	0	0	0	0	0	0	0	0	0	0	1.75
b_2	1	0	0	1	0	0	0	0	0	0	1	3.47
c_2	1	0	0	0	1	0	0	0	0	0	1	2.14
a_3	1	1	0	0	0	0	0	0	0	1	0	1.57
b_3	1	1	0	1	0	1	0	0	0	0	0	2.05
c_3	1	1	0	0	1	0	1	0	0	0	0	2.22
a_4	1	1	0	0	0	0	0	0	0	0	0	2.43
b_4	1	1	0	1	0	1	0	0	0	0	1	1.82
c_4	1	1	0	0	1	0	1	0	0	0	0	1.98
a_5	1	0	1	0	0	0	0	0	0	1	0	2.02
b_5	1	0	1	1	0	0	0	1	0	1	0	1.13
c_5	1	0	1	0	1	0	0	0	1	0	0	1.31
a_6	1	0	1	0	0	0	0	0	0	1	0	1.84
b_6	1	0	1	1	0	0	0	1	0	0	1	2.44
c_6	1	0	1	0	1	0	0	0	1	0	1	1.83

The meanings of the predictor coefficients

The vector $[\underset{(9\times 1)}{\underline{B}}, \underset{(3\times 1)}{\underline{\gamma}}]^t$, redefined as $\underset{(12\times 1)}{\underline{B}}$ in equation (18) of Chapter II, is the coefficient vector for the reparameterized model of the experimental design and, thus, the elements of $\underset{(12\times 1)}{\underline{B}}$ represent relative rather than absolute effects. The meanings of these coefficients can be deduced by reference to the treatment matrix of Table 2. Table 2 illustrates the method by which values were assigned to the regression predictors and, thus, indicates how the coefficients of these predictors should be interpreted.

The hypothetical condition of Product A at $1.99 with low competition and "zero" store size was used as the reference level. From the first row of $\underline{X}_\tau$, it can be seen that with this choice of conditions, the intercept variable is the only nonzero predictor. (The first row of $\underline{X}_\tau$ gives the predictor vector for the condition Product A at $1.99 with moderate competition and average store size. Altering this first row by setting X10, X11, and X12 at zero gives the predictor vector for the reference condition.) Because the dummy intercept variable is the only nonzero predictor in the reference condition, its coefficient represents the sales that would be expected given Product A at $1.99 with low competition and zero store size. Of course, β_1 is a hypothetical value since it pertains to stores with an average size of zero. Nevertheless, this hypothetical situation provides a useful reference against which to measure the relative effects of changes in the experimental variables and the covariates.

From $\underline{X}_\tau$, it can be seen that X2 and X3 are dummy variables which take on nonzero values only when the experimental variable "price" is set at \$2.25 or \$2.49, respectively. The coefficients β_2 and β_3, then, represent the "main effects," ceteris paribus, of price changes relative to the reference condition. That is, if all the requirements of the reference condition prevail with the exception that price is changed to \$2.25, then β_2 represents the change in sales that can be expected from the sales level of the reference condition. Similarly, β_3 represents the change in sales that would be expected by changing price from \$1.99 to \$2.49 while holding all other variables at the reference level. In the same fashion, β_4 and β_5 represent the main effects, ceteris paribus, for product changes from the reference level. β_4 represents the main effect on sales of Product B relative to Product A; and β_5 represents the main effect of Product C, again, relative to Product A.

As discussed in Chapter II, it was felt that price and product might not be independent of each other in their effects upon sales. It was felt that simultaneous changes of both price and product might not lead to an effect on sales equal to the addition of the separate, ceteris paribus effects of these two variables. Instead, in addition to these main effects, there might also be some synergistic or other type of interaction effect. To detect these possible interactions, the variables X6 through X9 are introduced into the model. For example, X6 takes on nonzero values when and only when both X2 and X4 take on nonzero values; that is, only when prices and product are simultaneously changed from their reference levels to \$2.25 and B, respectively. Reading row 5 of

$\underline{X}_T$, but assuming X10, X11 and X12 are at zero as in the reference condition, reveals that the total expected effect on sales caused by simultaneously varying price and product from their reference levels to the levels of \$2.25 and B is the sum of the two main effects β_2 and β_3 , and the interaction effect period. β_6, β_7, β_8 and β_9 are similarly defined for the other possible simultaneous variations of the experimental variables.

β_{10} and β_{11} are the coefficients of the predictor variables related to the covariate "competition." They respectively represent the effects of moderate and high competition relative to the reference treatment. Finally, β_{12} is the coefficient of the predictor variable "store size." X12 is not a dummy variable but a quantitative one, capable of taking on any value on the real number line. Because of this, β_{12} is interpreted as the partial derivative of sales with respect to store size. That is, a unit change in store size leads, <u>ceteris paribus</u>, to a change in expected sales equal to the magnitude of β_{12} .

The above interpretations of the individual elements of $\underline{B}$ result from the particular experimental conditions chosen to serve as a reference level and from the particular method of assigning values to the dummy predictors. The strategy employed here with respect to the reference level choice and method of assigning predictor values is valid but not unique. There are a multitude of other equally valid schemes. Had one of these other schemes been chosen, the interpretation of the various elements of $\underline{B}$ would have changed and differing numerical estimates for the β_i would have been arrived at below. Regardless of the scheme chosen, however, and provided that $\underline{X}_{\tau}$ was constructed in a manner consistent with the adopted scheme, the vector of estimated mean sales, $\hat{\underline{\mu}}$, would remain unaffected. The reason for this particular scheme's adoption was that the author felt it was easy to apply and offered straightforward insight as to the nature of the effects of the various predictor variables upon sales and the relationships of these effects to each other.

A note concerning the model

One final comment is made before the analysis proper. One can observe that there are no terms in the regression model to represent interactions between the covariates and the experimental variables or to represent interactions between store size and competition. As discussed in Chapter II, the appropriateness of the analysis of covariance model depends upon the assumption that there are no interaction effects between experimental variables and covariates. In this particular situation, there appeared to be no reason to feel that the assumption of independence of effects was untenable. Further, the effects of store size and competition were similarly felt to be unrelated. These two assumptions could be tested, provided the appropriate data were available.

To test the assumption of independence between covariates and experimental variables, one could segregate the sales data by experimental treatment and run nine separate regressions of sales against the covariates, without including the experimental variables as predictors. The results of these computations would be nine vectors of estimated coefficients which could be tested for equality. Indications of inequality among the nine vectors (intercept elements excluded) would be an indication of interactions between the experimental variables and the covariates. Inequalities would indicate that the effects of the covariates on sales would depend upon the experimental conditions rather than be the same for all treatments.

To test for interaction between the covariates, one could introduce two variables representing this interaction in the model of equation (5), Chapter II. (One interaction variable would be needed for each of the combinations X10-X12 and X11-X12.) The hypothesis that the resulting coefficients were not significantly different from zero could then be tested.

In order to perform either of these tests, however, there must be enough variation among the values of the covariate predictors in the data matrices of the appropriate regressions so that the cross-products matrices are not singular. With respect to the first test above, each experimental treatment appeared in only two different stores; therefore, there could be a maximum of only two different rows in the matrices of predictors for the nine separate regressions. Since these matrices would contain four columns, the cross-products matrices would be singular. Similarly, the lack of variation in the covariates made adding covariate interaction terms to the original model infeasible. Adding more stores to the experiment in an effort to provide greater variation in the data on covariates would have added greatly to the cost of the experiment. Management preferred to rely on a priori judgment and let these assumptions go untested rather than to incur the added expense necessary to test them.

The Analysis Proper

The analysis with nine-week data

Data were collected for nine weeks (through November 4) before any analysis was attempted. During these nine weeks, 135 usable observations were generated. Despite the efforts of the experiment's administrators, 27 potential observations were lost through field errors, such as failure, to hold to the experimental conditions and failure to correctly inventory and report sales.

The regression analysis

The usable observations were input to the SAS regression procedure along with the model for this experimental design. Table 5 contains the resultant vector of estimated regression coefficients.

TABLE 5

THE VECTOR OF ESTIMATED REGRESSION COEFFICIENTS, $\underline{\hat{B}}$ (NINE-WEEK DATA)

Coef.	Est. Value	Coef.	Est. Value	Coef.	Est. Value
β_1	5.480	β_5	-1.463	β_9	0.843
β_2	-1.158	β_6	2.665	β_{10}	-2.242
β_3	-1.867	β_7	1.811	β_{11}	-3.504
β_4	-3.029	β_8	1.779	β_{12}	0.012

Looking at Table 5, the first observations to be made concern the covariates' effects upon sales. As competition increases, the evidence is that sales tend to decrease as would be predicted a priori. β_{10} and β_{11}, the coefficients for moderate and high competition, are both negative in sign and, as would be expected, the coefficient for high competition is larger in absolute value than the coefficient for moderate competition. The effects of store size are also in the anticipated direction. All other things being equal, larger stores tend to sell more, as indicated by the sign of β_{12}. β_{12}, however, is smaller by two orders of magnitude than any other coefficient in the model. This means that store size is of little importance in comparison to the other variables thought to influence boxed chocolate sales. In fact, it appears that smaller stores as a group will probably outperform the larger ones on candy sales when all things are considered. This is because most of the bigger stores are located in larger shopping areas where candy competition tends to be high and, thus, the very small sales advantage large stores have due to their size will be swamped by the negative effects of the greater competition they face.

These empirical findings regarding the influences of the covariates upon sales are encouraging. The fact that these results are in line with a priori expectations supports the construct (logical) validity of the data and increases confidence that the relationships discovered below among the experimental factors are bona fide.

Turning attention to the coefficients related to the experimental variables, the effects of price and product upon sales are a bit muddled. The main effects associated with product, β_4 and β_5, indicate that Product A should outperform both Products B and C. This indication comes from the negative signs associated with both β_4 and β_5. The main effect parameters β_2 and β_3 indicate that the differential effects of higher prices relative to $1.99 are also negative. In addition, because β_3 is larger than β_2, it appears that sales are a monotonically decreasing function of price. These apparently clear-cut effects of both product and price become complex, however, upon consideration of the interaction effects embodied in the coefficients β_6 through β_9.

The nonzero values of the interaction coefficients indicate that the total effect of simultaneously varying price and product is different from the sum of the independent effects of these variables. Further, while the main effect parameters all have negative coefficients, meaning that ceteris paribus changes of price and product from the reference condition have a depressing effect upon sales, the interaction parameters are all positive. This may be interpreted as meaning that Products B and C react differently to price changes than Product A does. The empirical evidence indicates that Products B and C, unlike A, fail to strictly obey the traditional law of downward sloping demand. For both these products the relationship between price and sales is nonmonotonic within the range of prices tested. The evidence suggests an optimal price level with sales declining as price moves in either direction. (See the sales vector of Table 6 appearing on page 62.) Because A served as the reference level for the experimental variable "product" and because A did not exhibit nonmonotonic behavior with the price levels tested, the main effect parameters of price, measured with respect to A, did not reveal any "backward bending" characteristics. The fact that B and C react to price changes differently from A had to be revealed in the interaction terms of the regression model.

Observing a violation of the traditional inverse price-quantity relationship does not necessarily mean that the construct validity of the data is suspect. Similar empirical observations for other products have been made by other researchers.[3] In fact, there has been a great deal of discussion of late in the marketing literature concerning consumer's subjective perception of price and the effects of these perceptions upon price-quality and price-quantity relationships.[4] These results do suggest, however, that investigating other prices in an attempt to find the optimal price level could be worthwhile. Such an investigation was not carried out as part of this research.

It is worth noting here that Products B and C are produced by the same supplier and are more similar to each other than either of them is to Product A, produced by a different supplier. This may in part explain the discrepancy in the form of the price-quantity relationships of B and C and A. Of course, the failure to observe a nonmonotonic sales response for A does not mean that one does not exist. It may well be that the particular price levels investigated failed to uncover any existing "backward bending" characteristics for Product A. Again, the utility of investigating additional price levels is suggested.

Identifying the proper course of action

Table 6 contains the results of using the regression estimates above to predict the average sales of an average store for each of the nine experimental treatments. This table also contains the utility and differential utility vectors that result from successive transformations of the sales vector. The utility vector is derived by premultiplying the sales vector by the matrix $\underline{K}$, whose diagonal elements appear in Table 3. The differential utility vector is derived from $\underline{U}$ by subtracting in turn the utility of the optimal treatment from the utilities of each of the nonoptimal treatments.

TABLE 6

SALES, UTILITY AND DIFFERENTIAL UTILITY VECTORS
(NINE-WEEK DATA)

Treatment	Sales		Utility		Differential Utility
	Amount	Rank	Amount	Rank	
1	3.260	1	$731.60	1	. . .
2	2.102	3	644.93	3	-$ 86.67
3	1.393	6	533.32	4	- 198.28
4	0.231	7	48.12	9	- 683.47
5	1.738	5	505.69	5	- 225.91
6	0.143	8	52.39	8	- 679.21
7	1.797	4	374.77	6	- 356.83
8	2.450	2	712.88	2	- 18.72
9	0.773	9	283.65	7	- 447.94

Earlier, it was stressed that decision making should be based on the relative utilities of the various experimental treatments rather than on the raw treatment "qualities." The data of Table 6 point out the potential pitfall of ignoring this point. In this case, the top three treatments ranked by sales also turn out to be the top three treatments when the ranking is by utility. The one-to-one correspondence, however, stops there. The transformation from sales space to utility space caused all but one of the lower rankings to be reshuffled. This reshuffling could just as easily have encompassed the higher ranked treatments had the utility transformation matrix been different in character. Had this been the case, and if the decision were based on sales, an entirely inappropriate course of action most likely would have been followed.

Correctly concentrating on the utility vector, it can be seen that the optimal treatment, based on the data gathered in these first nine weeks, is Product A at $1.99. The expected utility to the firm of adopting this treatment is $731.60 per week. Product C at $2.25 ranks second with an expected utility of $712.88 per week, and the rest of the treatments lag behind. If terminal action is to be taken at this point, and the treatments tested are the only alternatives considered, Product A should be adopted and priced at $1.99. The question remaining is: Should terminal action be taken at this point?

Program THESIS was used to apply the methodology of Chapter II for determining the risk of a terminal decision and the value of further testing. Based on the above data, the expected value of perfect information, or the expected opportunity loss associated with choosing the first treatment for chainwide adoption, is $377.15 per week. (It should be emphasized that this figure represents the expected <u>opportunity</u> loss--the expected difference between the utility the firm would realize from adopting the first treatment and the utility it could realize if perfect knowledge were available--not an actual accounting loss.) The decision whether or not to make a terminal choice now depends upon the firm's ability to economically reduce this risk level through further testing.

Table 7 shows the expected value of sample information for continuing the experiment varying lengths of time.

TABLE 7

EVSI FOR EXPERIMENTS OF VARIOUS SIZES

Length of Exp. (Weeks)	EVSI	Length of Exp. (Weeks)	EVSI
1	$107.69	4	$210.97
2	157.61	5	228.17
3	188.64	6	241.75

It can be seen from Table 7 that the EVSI continues to increase, but at a decreasing rate, as the experiment is lengthened. Costs, of course, will also increase as the experiment is lengthened. Once the cost of obtaining sample information exceeds the expected value of that information, it is no longer economically worthwhile to gather data and the experiment should be terminated. Discussions with the firm's management led to a decision to continue the experiment for at least three more weeks, through November 25, before making a terminal decision. The analysis performed after this additional testing period is described in the next section.

<u>The analysis with twelve-week data</u>

Continuing the experiment for three additional weeks resulted in 45 more usable observations being generated. These data were combined with the previous data and the analysis procedure was repeated using the entire 180 observations.

The regression analysis

Table 8 contains the estimated regression coefficients for the updated analysis. For comparison purposes, the previous coefficient estimates are also included.

TABLE 8

REVISED VECTOR OF ESTIMATED REGRESSION COEFFICIENTS

Coef.	Prev. Est.	Rev. Est.	Coef.	Prev. Est.	Rev. Est.	Coef.	Prev. Est.	Rev. Est.
β_1	5.480	5.688	β_5	-1.463	-1.858	β_9	0.843	1.387
β_2	-1.158	-1.871	β_6	2.665	2.904	β_{10}	-2.242	-1.369
β_3	-1.867	-2.507	β_7	1.811	2.543	β_{11}	-3.504	-2.360
β_4	-3.029	-3.012	β_8	1.779	1.600	β_{12}	0.012	0.008

A comparison of the two estimated $\underline{B}$ vectors in Table 8 reveals that the coefficients of the covariates (β_{10}, β_{11} and β_{12}) within the revised vector bear approximately the same relative size relationships to each other as did the estimated covariate coefficients in the initial vector. The revised estimates, however, are all smaller than their counterparts. The portion of the revised coefficient vector pertaining to the experimental variables likewise contains the same internal relationships as are found in the initial vector; however, where the covariate coefficients tended to be smaller the experimental variable coefficients in the revised vector are, as a group, larger than their counterparts. Thus, some of the variation in sales that initially had been attributed to the covariates was, in this later analysis, attributed to the experimental factors.

In the revised vector, the interaction parameters are still important in size and positive in sign meaning that the new data are consistent with the earlier finding that Products B and C are subject to the backward sloping demand phenomenon. In summary, then, the new observations obtained by continuing the experiment three weeks confirm the regression relationships found in the earlier data.

Identifying the proper course of action

Table 9 contains the results of using the updated regression estimates to predict average sales and utilities. The earlier estimates are again provided for comparison.

The utility vector from the revised analysis reveals that Product A at $1.99 is still the optimal treatment. In fact, except for the eighth and ninth ranked treatments, all of the treatments maintained the same relative rankings. In the initial analysis, Treatments 4 and 6 had been close together in utility and the variance of the quantity $(\hat{u}_4 - \hat{u}_6)$ was large.* An interchange of these treatments' rankings is not surprising

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*The variance of $(\hat{u}_4 - \hat{u}_6)$ can be found by using data from Σ_{δ}, which appears in the Appendix, and employing the following identity:

$$\hat{\delta}_3 = \hat{u}_4 - \hat{u}_1 \; ; \; \hat{\delta}_5 = \hat{u}_6 - \hat{u}_1 \; ; \; \hat{\delta}_3 - \hat{\delta}_5 = \hat{u}_4 - \hat{u}_6 \, .$$

$$V(\hat{u}_4 - \hat{u}_6) = V(\hat{\delta}_3 - \hat{\delta}_5) = V(\hat{\delta}_3) + V(\hat{\delta}_5) - 2\,\mathrm{Cov}(\hat{\delta}_3, \hat{\delta}_5) \, .$$

TABLE 9

REVISED SALES, UTILITY AND DIFFERENTIAL UTILITY VECTORS

Treatment	Sales Estimates				Utility Estimates				Differential Utility Estimates	
	Initial		Revised		Initial		Revised			
	Amount	Rank	Amount	Rank	Amount	Rank	Amount	Rank	Initial	Revised
1	3.260	1	4.333	1	731.60	1	$972.59	1	$ . . .	$ . . .
2	2.102	3	2.462	4	644.93	3	755.63	3	- 86.67	-216.96
3	1.393	6	1.826	6	533.32	4	699.42	4	-198.28	-273.16
4	0.231	7	1.321	8	48.12	9	275.64	8	-683.47	-696.95
5	1.738	5	2.354	5	505.69	5	685.17	5	-225.91	-287.42
6	0.143	8	0.414	9	52.39	8	152.15	9	-679.21	-820.40
7	1.797	4	2.475	3	374.77	6	516.35	6	-356.83	-456.24
8	2.450	2	3.147	2	712.88	2	915.94	2	- 18.72	- 56.65
9	0.773	9	1.355	7	283.65	7	497.58	7	-447.94	-475.01

in view of this fact. Overall, the evidence is that the rankings in the utility vector are stable.

Again, THESIS was used to determine the new EVPI and the new EVSI for experiments of various sizes. With the added information, the EVPI dropped from $377.15 to $230.01 per week, indicating that the added sample information reduced the cost of uncertainty by $147.14 per week. Recall that prior to collecting the added information, the EVSI for continuing the experiment three weeks was $188.64. Considering the fact that only 45 additional observations were collected instead of the 54 planned for, there is reasonable agreement between the prior EVPI and the actual reduction in the risk associated with a terminal decision.

Table 10 contains the EVSI values for continuing the experiment varying lengths of time beyond the twelve weeks already analyzed. It can be seen from this table that these values have dropped considerably from their levels in the initial analysis. None of the revised values were felt to be large enough in comparison to experimental costs to justify continued data gatherings. Consequently, the experimental program was terminated.

TABLE 10

EVSI FOR EXPERIMENTS OF VARIOUS SIZES

Length of Exp. (Weeks)	EVSI	Length of Exp. (Weeks)	EVSI
1	$39.53	3	$ 86.64
2	67.70	4	100.92

Summary and Discussion of the Analysis

Summary of the Empirical Findings

Initially, 135 observations gathered over a nine-week period were used to estimate the reparameterized regression equation representing the experimental design of this study. This regression analysis revealed:

1. Evidence that competition's effect on candy sales is negative, as would be expected;
2. Evidence that larger stores tend to have higher boxed chocolate sales, as would be expected. However, the effect of store size is negligible in comparison to the effects of the other variables studied;
3. Evidence that important interactions exist between the effects of price and product upon sales. The price-quantity relationships for Products B and C appear to be nonmonotonic in the price range tested while Product A appears to follow the traditional law of downward sloping demand; and
4. Evidence that, all things considered, Product A, priced at $1.99, is the combination of experimental variables with the highest expected sales.

Transformation of the estimated average sales vector of the regression analysis to utility space identified Product A, priced at $1.99, as the optimal treatment with expected utility to the firm of $731.60 per week if the treatment is adopted chainwide. A risk analysis employing the concept of differential utility showed that the expected opportunity loss of adopting the optimal treatment chainwide was $377.15 per week. An examination of the expected values of sample information associated with continuing the experiment varying lengths of time revealed that this risk level could be reduced economically through further experimentation.

In accordance with the last finding above, an additional 45 observations were gathered by extending the experiment three weeks. After these additional data were gathered, the analysis procedure was repeated. The results of the revised regression analysis closely paralleled those of the earlier study. Transformation of the revised sales vector to utility space again revealed Product A at $1.99 as the best treatment and further revealed the treatment rankings within the utility vector as a whole to be stable. The risk analysis based on the entire twelve-week data showed that the expected opportunity loss of adopting the optimal treatment had dropped to a level of $230.01 per week. Examination of the EVSI for continuing the experiment indicated that this risk level could no longer be economically reduced by this experimental program. As a result, the experiment was terminated.

Comparison with Traditional Analysis

The analysis above differs markedly from the usual analysis of experimental data and, in a marketing decision context, should be preferred over the more traditional procedures. In a decision context, a choice must eventually be made. A suitable analysis procedure should identify the treatment to be chosen if and when terminal action is taken and should provide a criterion for deciding whether further informationgathering efforts are warranted. The analysis above satisfies both of these requirements; traditional methods often satisfy neither.

Traditionally, sales are the variable of focus and classical significance testing is the vehicle for analysis. For the experimental situation above, the usual null hypotheses would be (1) that the direct price effect parameters in the sales regression equation are all equal to zero (that is, that price has no direct effect on sales), (2) that the direct product effect parameters are all equal to zero and (3) that the interaction parameters are all equal to zero. F tests employing the appropriate quadratic forms[5] provide information regarding the probability of observing the sample results that were in fact observed, given the corresponding null hypotheses are true. The experimenter is then left to decide arbitrarily which probabilities are small enough to warrant discarding the associated hypotheses.

This type of analysis is incomplete in a decision context. First, and most obviously, differences in sales are not of direct interest. It is differences in utility among the various treatments that matters. Second, the focus should not be on the probability of utility differences but rather on the expected economic consequences of choosing one alternative over the others. Third, traditional procedures give no guidance with respect to what should be done if no significant differences are found.

Suppose F tests on the suitable transformed experimental data above yield no significant results. Should a choice be made anyway or should more data be gathered? The usual interpretation of insignificant results is that chance or unmeasured factors cannot be ruled out as the cause of variations among the treatments and, thus, no specific guidance is given as to which treatment is to be chosen. Deciding among the treatments, despite the lack of significant differences, obviates the need for any kind of significance test in the first place. This practice is similar to the tactic of appointing a committee to study a matter while knowing beforehand that the committee's recommendations will be ignored. If, on the other hand, a decision is made to gather more data, there is no way of judging beforehand whether or not the new data will lead to significant results nor is there a way of measuring the economic value of the additional information.

To alter the scenario, suppose instead that significant differences, due to one or more factors, are found. It is incorrect to automatically assume that the significant difference resides in the treatment with the highest mean utility.[6] Scheffé's test, Tukey's test or some similar procedure may or may not identify a single treatment as being significantly different from all the others; and if one is revealed, it may be significantly inferior rather than superior. Even if a treatment is revealed to be significantly better than its competitors based on the sample information at hand, classical procedures give no measure of the economic risk associated with choosing that treatment. All that given is the probability of a correct choice. Even if the probability a correct choice is high, if the economic consequences of a wrong decision can be severe, then a substantial amount of risk may be associated with the decision.

The analysis procedure that was employed here does not suffer from the above deficiencies. A clear indication is given as to which treatment is preferred given the information at hand and a measure of the economic risk associated with choosing that treatment is provided. Further, the expected value of additional information can be calculated and compared with the anticipated costs of that additional information. This provides a logical economic criterion for deciding whether to gather the additional information or to proceed with a terminal decision. Clearly, when analyzing experimental data within a decision framework, the Bayesian differential utility methods used here have an advantage over classical methods.

Two Parenthetical Comments

Before leaving the analysis, two comments are in order. First, the analysis is in terms of weekly utility for weeks in the September-November quarter of the year. As indicated by Table 11 on the next page, candy sales are very seasonable. While this seasonality will not affect the relative ranking of the various treatments, care must be taken to consider seasonality in attempts to project absolute performance data to longer periods of time.

Second, price in this analysis was treated as a fixed effect. Many would argue that since management is interested in the range of prices from \$2.00 to \$2.50, price should have been treated as a random effect. This study could probably have been improved by expanding the number of price levels considered but treating price as a random effect would not have materially improved the study. It might have even diminished the value of the study's findings as far as management was concerned and certainly would have added to the complexity of the analysis.

If the experimental results were to be analyzed considering price as a random effect, then consistency of logic would require that the levels of price to be tested be chosen randomly. Random selection of experimental levels is a prerequisite for validly generalizing experimental results to the entire population of possible effect levels. Random selection of price levels, however, could easily result in test prices so close together that demand differences could not be detected with any experiment of reasonable size. This would certainly impair the utility of the experiment for decision-making purposes.

TABLE 11

DISTRIBUTION OF RETAIL DEPARTMENT STORE CANDY SALES BY MONTH*

Month	% of Yearly Volume	Month	% of Yearly Volume
January	4.5	July	4.6
February	8.0	August	5.0
March	9.2**	September	5.8
April	9.7**	October	6.8
May	5.6	November	6.9
June	5.4	December	27.3

*Median figures for 1969 reported by National Retail Management Association.[7]

**Varies according to the date of Easter.

Even if, by chance, the randomly selected price levels were reasonably spaced, treating price as a random effect would complicate the analysis considerably. The distributional properties of a model involving only fixed effects or only random effects are straightforward. This is not the case for a mixed model involving both fixed and random effects. Distributional properties must be postulated for the mixed model's interaction terms and these assumptions affect the proper specification of the error term.[8] Since many different sets of distributional properties have been advanced as realistic,[9] from a logical problem-solving point of view, arbitrary treatment of price as a fixed effect seems no worse than arbitrary specification of the distributional properties of the interaction terms.

FOOTNOTES

[1]F. Ladik, L. Kent, and P. Nahl, "Test Marketing of New Consumer Products," Journal of Marketing, 24, No. 2 (April, 1960), pp. 29-34 at pp. 32-33.

[2]Jolayne Service, A User's Guide to the Statistical Analysis System (Raleigh, N. C.: North Carolina State University, 1972), pp. 90-120.

[3]See, for example, Z. V. Lambert, "Product Perception: An Important Variable in Price Strategy," Journal of Marketing, 34, No. 4 (October, 1970), pp. 68-76.

[4]For a comprehensive review of this literature, see Kent B. Monroe, "Buyers Subjective Perceptions of Price," Journal of Marketing Research, 10, No. 1 (February, 1973), pp. 70-80.

[5]Arthur S. Goldberger, Econometric Theory (New York: John Wiley and Sons, 1964), pp. 172-178.

[6]For a discussion of this point, see William Menderhall, The Design and Analysis of Experiment (Belmont, Calif.: Wadsworth Publishing Co., 1968), pp. 204-205.

[7]"How to Succeed with Candy," Stores, 53 (May, 1971), p. 12.

[8]F. Graybill, An Introduction to Linear Statistical Models (New York: McGraw-Hill, 1961), p. 396.

[9]See, for example, M. B. Wilk and O. Kempthorne, "Some Aspects of the Analysis of Factorial Experiments in a Completely Randomized Design," Annals of Mathematical Statistics, 27 (1956), pp. 950-984; H. Scheffé, "A 'Mixed Model' for the Analysis of Variance," Annals of Mathematical Statistics, 27 (1956), pp. 23-26; and H. Scheffé, "Alternative Models for Analysis of Variance," Annals of Mathematical Statistics, 27 (1956), pp. 251-271.

CHAPTER IV

SUMMARY AND DISCUSSION

Summary of the Research

In Chapter I it was pointed out that there is a need for an improved decision-oriented methodology for analysis that can be applied to experimental data. While experimentation is in a period of ascendancy in marketing, especially in the area of pure research, the more widespread use of the experimental technique in applied research is restrained somewhat by the inadequacies of traditional analysis procedures. Traditional procedures very often fail to clearly point toward the correct course of action that should be taken when a decision is necessary, and they also give inadequate guidance with respect to the problem of determining the total size of an experiment.

Many people have recognized these shortcomings and have suggested that applying the concepts of Bayesian decision theory to experimental analysis would increase markedly the utility of the experimental technique in the decision-making context. A necessary step from theory to application, however, is the development of suitable methodology and, while there is general agreement concerning the utility of Bayesian decision theory in this type of situation, the application of this theory has been stalled by the lack of appropriate methodology. This research was aimed at this methodology gap. Here a Bayesian methodology for analyzing data from a factorial design with covariates was developed by expanding upon the work that Schlaifer and others have done with the concept of differential utility.

The application of Bayesian concepts to realistic problems offer founders in the empirical problems of computing expected utilities. These problems largely result from the complex and multivariate nature of the probability and utility functions associated with realistic problems. However, as pointed out in Chapter II, the decision in many choice situations depends not upon the absolute utilities associated with the alternatives but rather upon their relative utilities. Defining a "differential utility" vector whose elements represent the unknown differences in utility between the optimal and various nonoptimal choices can reduce the analysis of a problem to a series of systematic transformations of probability distributions, followed by taking the expectation of a fairly "clean" loss function with respect to the transformed probability distribution. Here a contribution was made to the methodology of Bayesian analysis by developing in detail the procedures necessary for applying differential utility analysis to the data from a factorial design with covariates. To demonstrate the practicality of the developed methodology, it was applied to experimental data gathered to aid a firm in a product-assortment decision.

Briefly, a firm whose principal business is the operation of a chain of ice cream and sandwich shops became interested in the possibility of including in its product assortment a fancy boxed chocolate product. Before making a final decision as to what price-product combination to adopt chainwide, the firm wished to gather some information as to what customer response to various alternatives might be. Accordingly, a 3^2 factorial design with covariates to remove the effects of important nuisance variables was adopted as the design for a test-marketing program. Chapter III reports the findings from this experiment.

An initial differential utility analysis of nine weeks of test data identified the price-product combination with the highest expected utility for the firm and quantified the economic risk associated with adopting that combination chainwide. Further, the analysis revealed that the decision's risk level could be economically reduced through further testing; hence, the experiment was continued for an additional three weeks and a second differential utility analysis was performed. This later analysis identified as optimal the same price-product combination the first analysis had and, in general, confirmed all the findings of the first analysis. The additional data also served to lower the decision's risk level to a point where gains from further experimentation could not be expected to offset the costs of continuing the test-marketing program and, accordingly, the program was terminated.

Discussion

Without a doubt, many aspects of the empirical investigation undertaken as part of this research could have been improved upon. Some of the more important considerations along these lines are discussed below. The fact remains, however, that the methodology developed and employed here is an improvement over the methods of classical analysis and represents an early step along a path of inquiry that should be continued. Accordingly, some suggestions for future research are put forth below.

Limitations of the Empirical Study

Discussion here focuses on four major considerations, all of which have been mentioned before. They are brought together here to facilitate evaluation. These four considerations are: (1) the limited number of price levels investigated, (2) the omission of packaging and point-of-purchase promotion from the experiment, (3) the lack of a test for interaction between the covariates and experimental variables and (4) the unexplored seasonality factor.

The limited number of price levels

In the planning stages of the experiment, management placed boundaries on the range of prices at which they would consider offering any one of the three candidate products for sale. In the experiment, three price levels were investigated, the boundary values and a price halfway between. Three price levels were felt to be sufficient for detecting any nonlinearities over the 50-cent range of acceptable prices. The first analysis revealed not only a nonlinear relation between price and quantity demanded but a nonmonotonic one. With this evidence at hand, it could have been beneficial to the search for optimality to introduce other price levels to the investigation. Introducing new price levels while continuing the initial experiment would have meant adding new stores from different geographic areas to the study and would have meant steeply increasing the costs of the experimental program. Management did not feel that the value of the information to be gained from studying additional prices could offset the costs of expanding the experiment. Consequently, the experiment continued as before.

<u>Omitted decision variables</u>

As pointed out in Chapter I, before actually offering a boxed chocolate product chainwide, major decisions had to be made concerning the choice of actual physical product, the price at which the product would be offered, the packaging for the product and the point-of-purchase promotional support the product would receive. Management decided that only the first two decision areas were to be investigated in this experiment. This decision was based on the added costs and lengthened time horizon that would have been engendered by including the other two factors. The apparent savings from not including promotion and packaging in the first experiment might, however, represent a false economy. If, at some later date, management decides to experiment with packaging and promotion, they will have two choices: either take the price and product already selected as given and determine the effects of different packages and promotional materials upon this price-product combination or, else, try to identify the best overall price-product package promotion combination by conducting an experiment involving all these factors. If the first strategy is followed, the firm will never know if one of the initially rejected product or price levels with different promotion or packaging might not have proved superior to the adopted level. On the other hand, if the second strategy is followed, the firm will be performing the experiment initially rejected. In any event, whether future experimentation is conducted or not, if either promotional or packaging variations have a significant effect on sales and profits, it may well be that the money initially saved through truncating the experimental program is being lost now to the opportunity cost associated with suboptimal choices for these decision variables.

The covariance model's validity

The validity of the covariance model employed in this study depends upon the assumption of independence between the effects of the experimental variables and those of the covariates. While there was no reason to doubt the validity of this assumption in this instance, it would certainly be more thorough to empirically test this assumption if possible. The methodology of such a test exists but could not be employed here because of data limitations. Briefly, the test requires estimating and comparing the parameters of nine regressions of sales against the study's covariates, one regression for each experimental treatment. If the hypothesis that the coefficient vectors for the different regressions are the same (except for the intercept) is rejected, then it must be assumed that interactions between covariates and treatment variables caused the inequalities.

In order to estimate the parameters of any one of these regressions, data would have to be available from at least four test stores receiving the same experimental treatment but having nonidentical values for the covariates. This requirement comes from the fact that the matrix of predictors for each regression will have four columns--one for each predictor and one for an intercept--and, consequently, must have at least four unique rows if the regression is to be estimated. If there are less than four unique rows, the cross-products matrix will be singular and the regression unestimable. The fact that each experimental treatment in this study appeared in only two stores ruled out the performance of this test. The firm's management was alerted to this situation before the test began but elected to accept the independence assumption of faith rather than expand the experiment.

The seasonality factor

As indicated in Table 11 of Chapter III, candy is a seasonable item. In the analysis conducted here, sales and utility were expressed in weekly amounts. If these figures are to be projected to longer periods of time, such as a year, the seasonality factor must definitely be considered in judging the absolute utility of any one experimental treatment. However, in trying to choose among the treatments (that is, in judging their relative utilities), seasonality becomes much less critical and perhaps insignificant. The three products under consideration are similar in nature and, thus, all should be affected in approximately the same manner by the seasonality factor. Thus, when the products are compared against one another, the effects of seasonality upon absolute utilities will be canceled out by the comparison process.

Directions for Further Research

In this final section of the dissertation, attention is directed toward areas in which the existing differential utility methodology for analyzing experimental data could be improved. Three areas are singled out: (1) expanding the methodology by applying the concepts used in this research to develop the analysis procedures for data from other experimental designs, (2) developing procedures for eliminating obviously inferior alternatives from further consideration in an ongoing experimental program and (3) incorporating in the methodology procedures for quantifying and including in the analysis the initial judgments of management concerning the relative worths of the various alternatives.

Expanding to other designs

In this research, attention was focused on developing analysis procedures for the type of experiment that seemed called for by the participating company's situation. However, the same general procedures used here could be adapted to deal with other experimental designs. To extend this methodology to a new design, all that need be done is to specify a correct, estimable regression equation for the design. With this step completed, it is a simple matter to develop the mean vector and covariance matrix for the qualities of the design's various treatments and to estimate these quantities from data. In fact, program THESIS can be used to perform a differential utility analysis for any experimental design. All that is needed are the correct design matrix, treatment matrix, utility transformation matrix, and data-dependent regression inputs.

Discarding inferior treatments

Another area that deserves investigation is the development of procedures for the dropping of obvious inferior treatments as the experimental program proceeds. This would allow relatively more observations to be devoted to the remaining treatments and should, thus, enhance the economics of experimentation. A key consideration in the development of such procedures is the fact that if observations are to be taken on only a subset of the experimental treatments, then mathematical complications are introduced to the derivation of $\underline{\Sigma}''_{\delta}$, the quantity needed in the computation of the expected value of sample information.[1] Such difficulties are not insurmountable, however.

Including subjective judgment

Finally, it is felt that quantifying and including in the analysis procedure the initial judgments of management concerning the relative utilities of the various experimental treatments would represent an improvement. It is often difficult, however, particularly in multivariate situations, for managers to translate their judgments into accurate and consistent mathematical form. In this methodology, two things must be estimated, a mean vector and a covariance matrix. The ability to apply the transformations outlined in this research means that the mean vector estimated may be any one of $\underline{B}$, $\underline{\mu}$, $\underline{U}$ or $\underline{\Delta}$, whichever management feels most comfortable dealing with. Likewise, the covariance matrix may be $\underline{\Sigma}_b$, $\underline{\Sigma}_{\mu}$, $\underline{\Sigma}_u$ or $\underline{\Sigma}_{\delta}$ and there is no requirement that the covariance matrix estimated be the one corresponding to the mean vector estimated. In estimating the covariance matrix, however, care must be taken to insure the estimates of the individual elements of the matrix are consistent with each other. Some suggestions for insuring this consistency have previously been made[2] and, depending upon the statistical sophistication of the management involved, it should not be a difficult task to integrate one of the suggested estimation procedures into the methodology and, thus, formally incorporate prior managerial judgment into the analysis.

FOOTNOTES

[1]Howard Raiffa and Robert Schlaifer, Applied Statistical Decision Theory (Cambridge, Mass.: M.I.T. Press, 1968), pp. 146-148; 328-333.

[2]See, for example, D. G. Frederick and D. J. Laughhunn, "The Subjective Specification of Multivariate Parameters," Proceedings, First Northeast Regional Conference, American Institute for Decision Sciences, Chestnut Hill, MA, 1972, pp. 312-317; and Robert Winkler, "The Quantification of Judgment: Some Methodological Suggestions," Journal of the American Statistical Association (December, 1967), pp. 1105-1120.

APPENDIX

THE COMPUTER PROGRAMS EMPLOYED

The SAS Program

The Statistical Analysis System (SAS), developed at North Carolina State University by A. J. Barr and J. H. Goodnight, is an integrated collection of batch processing programs for performing statistical analyses of data. The number of options offered runs the gamut from simple descriptive statistics to multivariate procedures, such as discriminant and factor analysis. Also included are options for performing various nonparametric analyses. In this research, the "REGR" procedure from SAS was used to perform the regression portions of the analysis of Chapter III. This procedure is fully explained in A User's Guide to the Statistical Analysis System.[1]

To use the SAS REGR procedure, it was necessary only to specify the regression model, select the desired output and read in the data. The output asked for in this research included: (1) the matrix $(\underline{X}^t\underline{X})^{-1}$, (2) the analysis-of-variance table (necessary to get a printout of the estimated error variance) and (3) the vector of regression coefficients, $\underline{B}$. Multiplying $(\underline{X}^t\underline{X})^{-1}$ by the error variance provided the covariance matrix of $\underline{B}$, $\underline{\Sigma}_b$. Figures 1 and 2 are copies of the SAS output obtained using data from the first nine weeks of experimentation. Table 12 contains the matrix $\underline{\Sigma}_b$ derived from the information in Figures 1 and 2.

FIGURE 1

THESIS REGRESSION RUNS

ANALYSIS OF VARIANCE TABLE , REGRESSION COEFFICIENTS , AND STATISTICS OF FIT FOR DEPENDENT VARIABLE X13

SOURCE	DF	SUM OF SQUARES	MEAN SQUARE	F VALUE	PROB > F	R-SQUARE	C.V.
REGRESSION	11	570.26768210	51.84251655	3.85545	0.0002	0.25639312	83.48038 %
ERROR	123	1653.92491049	13.44654399			STD DEV	X13 MEAN
CORRECTED TOTAL	134	2224.19259259				3.66695296	4.39259

SOURCE	DF	SEQUENTIAL SS	F VALUE	PROB > F	PARTIAL SS	F VALUE	PROB > F
X2	1	88.18107186	6.55790	0.0113	6.78047714	0.50425	0.5140
X3	1	111.55389773	8.29610	0.0049	10.72338258	0.79748	0.6229
X4	1	101.81540204	7.57186	0.0069	62.13129761	4.62061	0.0315
X5	1	28.38260644	2.11077	0.1450	11.08620223	0.82446	0.6312
X6	1	0.07488979	0.00557	0.9387	18.66801335	1.38831	0.2392
X7	1	70.75672693	5.26208	0.0221	8.28522924	0.61616	0.5600
X8	1	0.85613613	0.06367	0.7968	8.57860611	0.63798	0.5683
X9	1	27.95232387	2.07877	0.1481	1.60438321	0.11932	0.7304
X10	1	27.17765748	2.02116	0.1539	29.85068791	2.21995	0.1349
X11	1	92.51036458	6.87986	0.0096	98.89289841	7.35452	0.0076
X12	1	21.00660526	1.56223	0.2111	21.00660526	1.56223	0.2111

SOURCE	B VALUES	T FOR H0:B=0	PROB > |T|	STD ERR B	STD B VALUES
INTERCEPT	5.48018724	2.82625	0.0057	1.93903469	0.0
X2	-1.15823527	-0.71011	0.5140	1.63106773	-0.13210213
X3	-1.86657012	-0.89302	0.6229	2.09018029	-0.21906929
X4	-3.02877193	-2.14956	0.0315	1.40901851	-0.35718543
X5	-1.46271997	-0.90800	0.6312	1.61092430	-0.16455077
X6	2.66484687	1.17827	0.2392	2.26166639	0.21220355
X7	1.81069906	0.78496	0.5600	2.30674374	0.11682834
X8	1.77909383	0.79874	0.5683	2.22738775	0.14167044
X9	0.84292490	0.34542	0.7304	2.44028285	0.06331294
X10	-2.24170711	-1.48995	0.1349	1.50455143	-0.23973196
X11	-3.50355454	-2.71192	0.0076	1.29190824	-0.39692793
X12	0.01243201	1.24989	0.2111	0.00994646	0.17934370

FIGURE 2

THESIS REGRESSION RUNS

THE X'X INVERSE MATRIX , RANK = 12

	INTERCPT	X2	X3	X4	X5	X6
INTERCPT	0.27961501	0.00601408	0.03479930	-0.04877100	-0.16321066	-0.06476056
	X7	X8	X9	X10	X11	X12
	0.14936432	-0.06301867	-0.02267860	-0.07620763	0.11662947	-0.00123763
	INTERCPT	X2	X3	X4	X5	X6
X2	0.00601408	0.19784875	0.18116899	0.08623592	0.04548843	-0.21787910
	X7	X8	X9	X10	X11	X12
	-0.14825160	-0.15216465	-0.17548860	-0.09857328	0.02880403	-0.00046038
	INTERCPT	X2	X3	X4	X5	X6
X3	0.03479930	0.18116899	0.32490532	0.09574772	0.04099963	-0.20749944
	X7	X8	X9	X10	X11	X12
	-0.11828981	-0.26030119	-0.31637737	-0.17933528	0.03034936	-0.00063150
	INTERCPT	X2	X3	X4	X5	X6
X4	-0.04877100	0.08623592	0.09574772	0.14764635	0.07590635	-0.15191465
	X7	X8	X9	X10	X11	X12
	-0.08379231	-0.15414253	-0.09236192	-0.02105196	-0.01302545	-0.00013469
	INTERCPT	X2	X3	X4	X5	X6
X5	-0.16321066	0.04548843	0.04099963	0.07590635	0.19299212	-0.01545582
	X7	X8	X9	X10	X11	X12
	-0.19508936	-0.01933190	-0.10618607	0.01719407	-0.08448460	0.00054563
	INTERCPT	X2	X3	X4	X5	X6
X6	-0.06476056	-0.21787910	-0.20749944	-0.15191465	-0.01545582	0.38040517
	X7	X8	X9	X10	X11	X12

	0.12030361	0.25100850	0.19898927	0.11643255	-0.06906294	0.00080962
	INTERCPT	X2	X3	X4	X5	X6
X7	0.14936432	-0.14825160	-0.11828981	-0.08379231	-0.19508936	0.12030361
	X7	X8	X9	X10	X11	X12
	0.39572002	0.05899735	0.18149845	0.05809948	0.09130828	-0.00046331
	INTERCPT	X2	X3	X4	X5	X6
X8	-0.06301867	-0.15216465	-0.26030119	-0.15414253	-0.01933190	0.25100850
	X7	X8	X9	X10	X11	X12
	0.05899735	0.36896144	0.25144842	0.11066195	-0.06256152	0.00079926
	INTERCPT	X2	X3	X4	X5	X6
X9	-0.02267860	-0.17548860	-0.31637737	-0.09236192	-0.10618607	0.19898927
	X7	X8	X9	X10	X11	X12
	0.18149845	0.25144842	0.44286327	0.17255513	-0.02828297	0.00055945
	INTERCPT	X2	X3	X4	X5	X6
X10	-0.07620763	-0.09857328	-0.17933528	-0.02105196	0.01719407	0.11643255
	X7	X8	X9	X10	X11	X12
	0.05809948	0.11066195	0.17255513	0.16834623	-0.01353153	0.00045304
	INTERCPT	X2	X3	X4	X5	X6
X11	0.11662947	0.02880403	0.03034936	-0.01302545	-0.08448460	-0.06906294
	X7	X8	X9	X10	X11	X12
	0.09130828	-0.06256152	-0.02828297	-0.01353153	0.12412311	-0.00069334
	INTERCPT	X2	X3	X4	X5	X6
X12	-0.00123763	-0.00046038	-0.00063150	-0.00013469	0.00054563	0.00080962
	X7	X8	X9	X10	X11	X12
	-0.00046331	0.00079926	0.00055945	0.00045304	-0.00069334	0.00000736

TABLE 12

COVARIANCE MATRIX, $\underset{\sim}{\Sigma}_b$ (NINE-WEEK DATA)

	Int.	X2	X3	X4	X5	X6	X7	X8	X9	X10	X11	X12
Int.	3.765	0.081	0.471	-0.659	2.192	-0.874	2.004	-0.847	-0.309	-1.022	1.573	-0.013
X2	0.081	2.663	2.434	1.156	0.605	-2.931	-1.990	-2.044	-2.353	-1.331	0.390	0.000
X3	0.471	2.434	4.370	1.291	0.551	-2.784	-1.587	-3.496	-4.249	-2.407	0.403	-0.013
X4	-0.659	1.156	1.291	1.990	1.022	-2.044	-1.130	-2.071	-1.237	-0.282	-0.175	0.000
X5	-2.192	0.605	0.551	1.022	2.595	-0.202	-2.622	-0.255	-1.425	0.229	-1.130	0.013
X6	-0.874	-2.931	-2.784	-2.044	-0.202	5.110	1.614	3.375	2.676	1.560	-0.928	0.013
X7	2.004	-1.990	-1.587	-1.130	-2.622	1.614	5.325	0.793	2.434	0.780	1.224	0.000
X8	-0.847	-2.044	-3.496	-2.071	-0.255	3.375	0.793	4.962	3.375	1.493	-0.847	0.013
X9	-0.309	-2.353	-4.249	-1.237	-1.425	2.676	2.434	3.375	5.957	2.326	-0.377	0.013
X10	-1.022	-1.331	-2.407	-0.282	0.229	1.560	0.780	1.493	2.326	2.259	-0.188	0.000
X11	1.573	0.390	0.403	0.175	-1.130	-0.928	1.224	-0.847	-0.377	-0.188	1.667	-0.013
X12	-0.013	0.000	-0.013	0.000	0.013	0.013	0.000	0.013	0.013	0.000	-0.013	0.000

Figures 3 and 4 and Table 13 contain the SAS output and corresponding $\underline{\Sigma}_b$ obtained using the entire twelve-week data.

Program THESIS

To perform the actual differential utility analyses of Chapter III, a time-sharing program, entitled THESIS, was written in the version of the language BASIC available at the University of Massachusetts Computer Center. Figures 5 through 11 below contain a flowchart of THESIS, a dictionary of variables for the program, a program listing, and copies of the program inputs and outputs for both the nine- and twelve-week analyses.

One comment on the program listing in Figure 7 is in order. As the program appears there, it will terminate a run after computing the expected value of sample information associated with continuing the experiment one week. To generate the EVSI associated with continuing the experiment for longer periods of time, it is necessary to insert, in the positions indicated by their line numbers, the following two lines of programming:

```
303 GO TO 345
376 MAT S = (1/K)*S .
```

In line 376, the number of weeks of experimentation for which the EVSI is desired (2, 3, ...) is substituted for K in the actual running of the program.

FIGURE 3

THESIS REGRESSION RUNS

ANALYSIS OF VARIANCE TABLE , REGRESSION COEFFICIENTS , AND STATISTICS OF FIT FOR DEPENDENT VARIABLE X13

SOURCE	DF	SUM OF SQUARES	MEAN SQUARE	F VALUE	PROB > F	R-SQUARE	C.V.
REGRESSION	11	523.75466646	47.61406059	4.10992	0.0001	0.21204118	86.65708 %
ERROR	168	1946.30644465	11.58515741			STD DEV	X13 MEAN
CORRECTED TOTAL	179	2470.06111111				3.40369761	3.92778

SOURCE	DF	SEQUENTIAL SS	F VALUE	PROB > F	PARTIAL SS	F VALUE	PROB > F
X2	1	67.08378976	5.79049	0.0163	23.13915366	1.99731	0.1557
X3	1	136.37819066	11.77180	0.0011	25.72361165	2.22039	0.1341
X4	1	104.46016912	9.01672	0.0034	78.53389203	6.77884	0.0098
X5	1	24.90232906	2.14950	0.1406	23.52597555	2.03070	0.1522
X6	1	2.01909444	0.17428	0.6803	29.61859413	2.55660	0.1076
X7	1	72.48566344	6.25677	0.0128	21.65782945	1.86945	0.1698
X8	1	0.43801996	0.03781	0.8404	9.09709581	0.78524	0.6195
X9	1	32.40465181	2.79708	0.0923	5.73254088	0.49482	0.5101
X10	1	12.84817050	1.10902	0.2940	15.37411823	1.32705	0.2495
X11	1	58.13695267	5.01823	0.0248	61.14421263	5.27781	0.0215
X12	1	12.59763505	1.08739	0.2989	12.59763505	1.08739	0.2989

SOURCE	B VALUES	T FOR H0:B=0	PROB > |T|	STD ERR B	STD B VALUES
INTERCEPT	5.68779132	3.66373	0.0006	1.55246078	0.0
X2	-1.87068137	-1.41326	0.1557	1.32366186	-0.23490903
X3	-2.50688323	-1.49010	0.1341	1.68236065	-0.32157394
X4	-3.01234548	-2.60362	0.0098	1.15698356	-0.39186734
X5	-1.85774517	-1.42503	0.1522	1.30365685	-0.22730171
X6	2.90435579	1.59894	0.1076	1.81642958	0.25680271
X7	2.54282217	1.36728	0.1698	1.85977110	0.17768706
X8	1.59973475	0.88614	0.6195	1.80529244	0.14144831
X9	1.38713141	0.70343	0.5101	1.97194589	0.10950798
X10	-1.36920836	-1.15198	0.2495	1.18857175	-0.16121697
X11	-2.36034458	-2.29735	0.0215	1.02742162	-0.29198965
X12	0.00820111	1.04278	0.2989	0.00786464	0.13301133

FIGURE 4

THESIS REGRESSION RUNS

THE X'X INVERSE MATRIX , RANK = 12

	INTERCPT	X2	X3	X4	X5	X6
INTERCPT	0.20803640	-0.00222954	0.01847282	-0.04002147	-0.12528952	-0.04019726
	X7	X8	X9	X10	X11	X12
	0.11597933	-0.04119070	-0.00680260	-0.05459690	0.08467361	-0.00089255
	INTERCPT	X2	X3	X4	X5	X6
X2	-0.00222854	0.15123495	0.13912318	0.07082859	0.03946197	-0.16635192
	X7	X8	X9	X10	X11	X12
	-0.11393872	-0.11993154	-0.13423808	-0.07168996	0.02139391	-0.00033853
	INTERCPT	X2	X3	X4	X5	X6
X3	0.01847282	0.13912318	0.24430720	0.07831075	0.03608901	-0.15915145
	X7	X8	X9	X10	X11	X12
	-0.09189384	-0.20045907	-0.23734432	-0.13024647	0.02241932	-0.00046236
	INTERCPT	X2	X3	X4	X5	X6
X4	-0.04002147	0.07082859	0.07831075	0.11554534	0.06112592	-0.11984953
	X7	X8	X9	X10	X11	X12
	-0.06717270	-0.12131126	-0.07600274	-0.01662690	-0.00890613	-0.00011247
	INTERCPT	X2	X3	X4	X5	X6
X5	-0.12528952	0.03946197	0.03608901	0.06112592	0.14669815	-0.01842811
	X7	X8	X9	X10	X11	X12
	-0.14839933	-0.01958946	-0.08573703	0.01262318	-0.06120821	0.00039758
	INTERCPT	X2	X3	X4	X5	X6
X6	-0.04019726	-0.16635192	-0.15915145	-0.11984953	-0.01842811	0.28479686
	X7	X8	X9	X10	X11	X12

	0.09457732	0.19293709	0.15111565	0.08526409	-0.04938436	0.00059231
	INTERCPT	X2	X3	X4	X5	X6
X7	0.11597933	-0.11393872	-0.09189384	-0.06717270	-0.14839933	0.09457732
	X7	X8	X9	X10	X11	X12
	0.29854998	0.04900347	0.14035500	0.04176531	0.06664694	-0.00034189
	INTERCPT	X2	X3	X4	X5	X6
X8	-0.04119070	-0.11993154	-0.20045907	-0.12131126	-0.01958946	0.19293709
	X7	X8	X9	X10	X11	X12
	0.04900347	0.28131519	0.19221977	0.08294232	-0.04700769	0.00059826
	INTERCPT	X2	X3	X4	X5	X6
X9	-0.00680260	-0.13423808	-0.23734432	-0.07600274	-0.08573703	0.15111565
	X7	X8	X9	X10	X11	X12
	0.14035500	0.19221977	0.33565108	0.12505895	-0.01763925	0.00039256
	INTERCPT	X2	X3	X4	X5	X6
X10	-0.05459690	-0.07168996	-0.13024647	-0.01662690	0.01262318	0.08526409
	X7	X8	X9	X10	X11	X12
	0.04176531	0.08294232	0.12505895	0.12194075	-0.00953809	0.00032658
	INTERCPT	X2	X3	X4	X5	X6
X11	0.08467361	0.02134391	0.02241932	-0.00890613	-0.06120821	-0.04938436
	X7	X8	X9	X10	X11	X12
	0.06664694	-0.04700769	-0.01763925	-0.00953809	0.09111617	-0.00050649
	INTERCPT	X2	X3	X4	X5	X6
X12	-0.00089255	-0.00033453	-0.00046236	-0.00011247	0.00039758	0.00059231
	X7	X8	X9	X10	X11	X12
	-0.00034189	0.00059826	0.00039256	0.00032658	-0.00050649	0.00000534

TABLE 13

COVARIANCE MATRIX, $\underline{\Sigma}_b$ (TWELVE-WEEK DATA)

	Int.	X2	X3	X4	X5	X6	X7	X8	X9	X10	X11	X12
Int.	2.410	-0.230	0.209	-0.463	-1.448	-0.463	1.344	-0.475	-0.081	-0.637	0.985	-0.012
X2	-0.230	1.749	1.610	0.823	0.452	-1.923	-1.321	-1.390	-1.552	-0.834	0.243	0.000
X3	0.209	1.610	2.828	0.904	0.417	-1.842	-1.066	-2.317	-2.746	-1.506	0.255	0.000
X4	-0.463	0.823	0.904	1.344	0.707	-1.390	-0.776	-1.402	-0.880	-0.197	0.104	0.000
X5	-1.448	0.452	0.417	0.707	1.703	-0.209	-1.715	-0.232	-0.996	0.151	-0.707	0.000
X6	-0.463	-1.923	-1.842	-1.390	-0.209	3.302	1.101	2.236	1.749	0.985	-0.568	0.012
X7	1.344	-1.321	-1.066	-0.776	-1.715	1.101	3.464	0.568	1.622	0.487	0.776	0.000
X8	-0.475	-1.390	-2.317	-1.402	-0.232	2.236	0.568	3.255	2.224	0.962	-0.544	0.012
X9	-0.081	-1.552	-1.746	-0.880	-0.996	1.749	1.622	2.224	3.893	1.448	-0.209	0.000
X10	-0.637	-0.834	-1.506	-0.197	0.151	0.985	0.487	0.962	1.448	1.413	-0.116	0.000
X11	0.985	0.243	0.255	0.104	-0.707	-0.568	0.776	-0.544	-0.209	-0.116	1.054	-0.012
X12	-0.012	0.000	0.000	0.000	0.000	0.012	0.000	0.012	0.000	0.000	0.012	0.000

FIGURE 5

FLOWCHART FOR THESIS

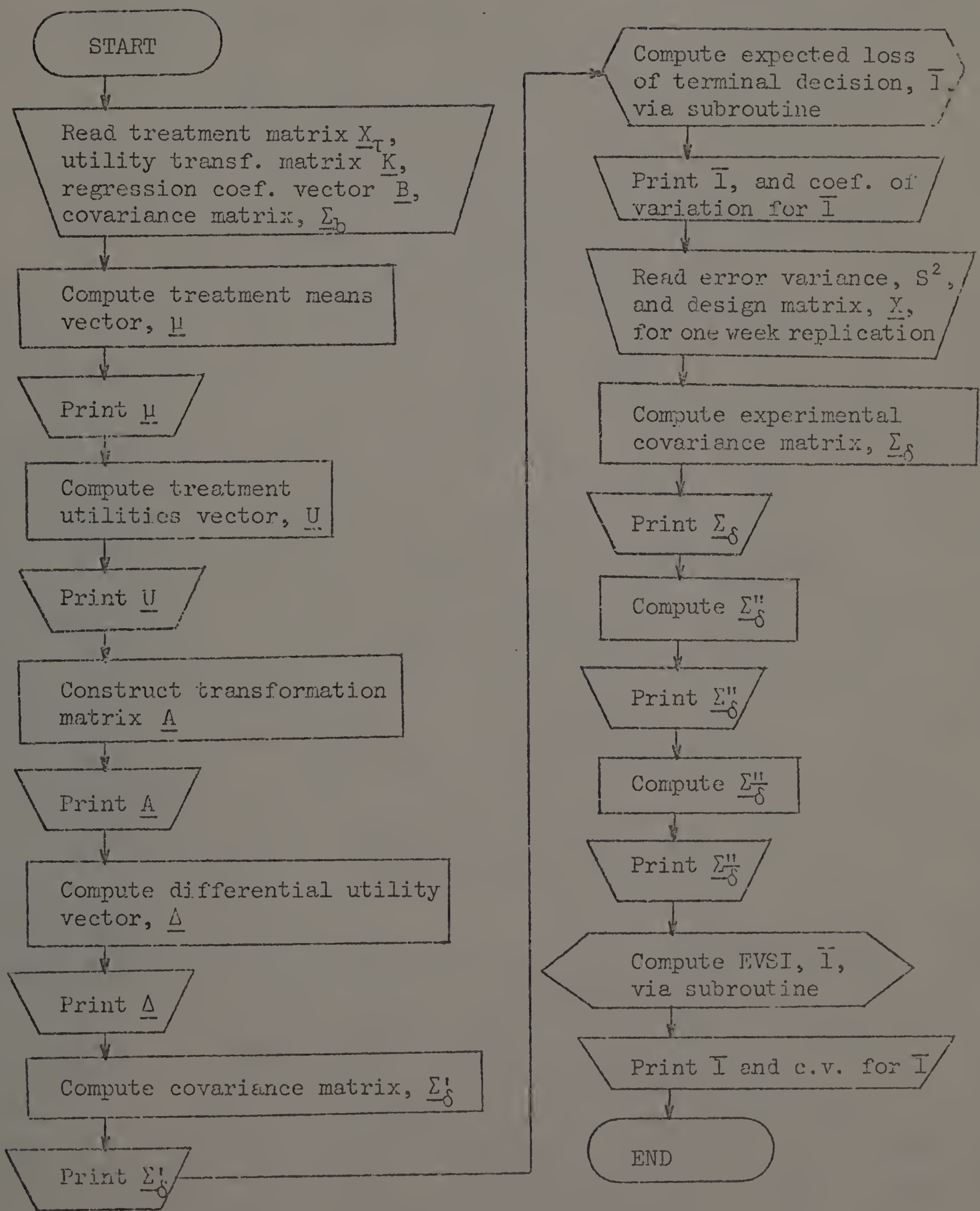

FIGURE 5--Continued

Subroutine

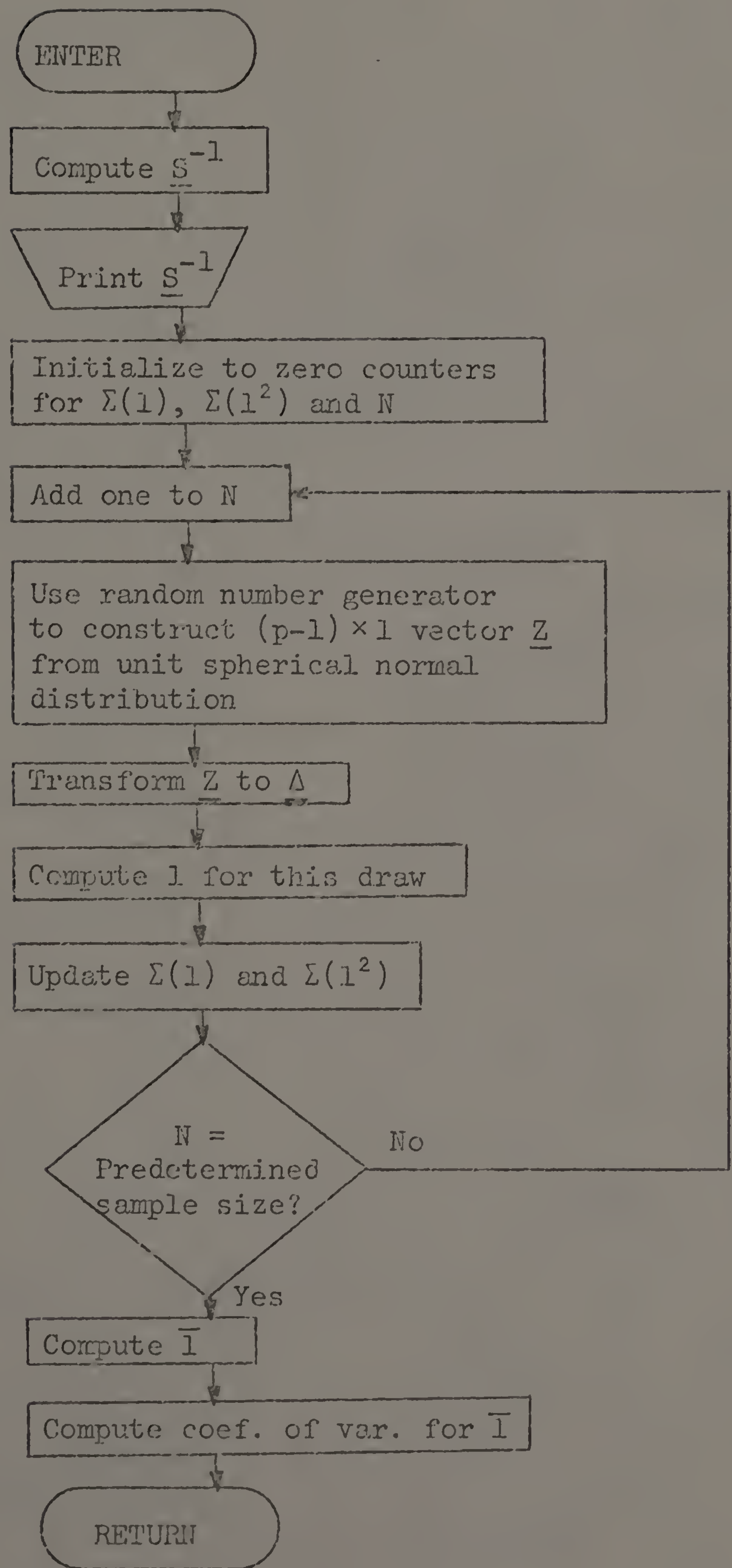

FIGURE 6

DICTIONARY OF VARIABLES

A(8,9)	=	The transformation matrix for moving from utility space to differential utility space (Chapter II, eq. (7)).
B(12,1)	=	The beta vector (Chapter II, eq. (18)).
C(9,8)	=	An intermediate matrix containing $(M^t A^t)$.
C1	=	Coefficient of variation for the distribution of 1.
D(8,1)	=	The differential utility vector (Chapter II, eq. (7)).
E(8,8)	=	An intermediate matrix containing the prior precision of $\underline{\Delta}$ in line 440, the posterior precision of $\underline{\Delta}$ in line 445, and S^{-1} (Chapter II, eq. (22)) in line 2120.
F(18,12)	=	The design matrix for a one-week run of the experiment.
G(12,18)	=	An intermediate matrix containing F^t.
H(12,12)	=	An intermediate matrix containing $(F^t F)$.
I	=	A counter.
I1	=	A counter.
J	=	A counter.
K	=	A counter.
L(9,9)	=	Used in an earlier version of the program but now superfluous.
M(9,9)	=	The utility transformation matrix, $\underline{K}$ (Chapter II, eq. (6)).
N(8,1)	=	An intermediate matrix containing $(\underline{S}^{-1}\ \underline{Z})$ (Chapter II, eq. (22)).
N1	=	The number of observations used to compute 1.
O(9,1)	=	The vector of treatment means, $\underline{\mu}$ (Chapter II, eq. (18)).
P(8,9)	=	An intermediate matrix containing $(\underline{A}\ \underline{M})$.

Q(8,8) = The experimental covariance matrix of $\underline{\Delta}$, Σ_{δ} in line 400 and the posterior covariance matrix of $\underline{\Delta}$, Σ''_{δ} in line 446.

Q1 = An intermediate variable used in constructing S^{-1} (Chapter II, eq. (22)).

R(9,12) = An intermediate matrix containing $(\underline{X}\ \underline{S})$.

S(12,12) = The covariance matrix of $\underline{B}$, Σ_{b} (Chapter II, eq. (20b)).

T(8,9) = Used in an earlier version of the program but now superfluous.

T1 = $\overline{1}$.

T2 = $\Sigma(1)$.

U(8,8) = An intermediate matrix containing the experimental precision of $\underline{\Delta}$.

U1 = The variance of $\overline{1}$.

U2 = $\Sigma(1^2)$.

V(8,8) = The prior covariance matrix of $\underline{\Delta}$, Σ_{δ} in line 280 and the preposterior covariance matrix of $\underline{\Delta}$, Σ_{δ} in line 459.

W(12,9) = An intermediate matrix containing X^t.

X(9,12) = The treatment matrix, $\underline{X}_{\tau}$ (Chapter II, eq. (6)).

Y(9,1) = The utility vector, $\underline{U}$ (Chapter II, eq. (6)).

Z(8,1) = The vector of random observations from the unit spherical normal distribution (Chapter II, eq. (22)).

FIGURE 7

PROGRAM LISTING FOR THESIS

```
1 REM THIS PROGRAM USES AS INPUTS THE B VECTOR, THE COVARIANCE
2 REM MATRIX, AND THE ERROR VARIANCE FROM A REGRESSION ANALYSIS.
3 REM OTHER INPUTS ARE THE TREATMENT MATRIX, THE UTILITY TRANS.
4 REM MATRIX, AND THE DATA MATRIX FOR ONE EXPERIMENTAL RUN.
5 REM CHECK REM STATEMENTS IN PROGRAM TO FIND MATRIX DIMENSIONS.
10 DIM X(9,12),M(9,9),B(12,1),S(12,12),Y(9,1),A(8,9),D(8,1),L(9,9)
15 DIM C(9,8),W(12,9),E(8,8),Z(8,1),F(18,12),G(12,18),H(12,12)
20 DIM Q(8,8),V(8,8),O(9,1),T(8,9),P(8,9),R(9,12),U(8,8),N(8,1)
30 REM TREATMENT MATRIX X(TREATMENTS (T) X PREDICTORS (P)),
31 REM UTILITY MATRIX M(T X T), BETA VECTOR B(P X 1),
32 REM COVARIANCE MATRIX S(P X P)
35 MAT READ X,M,B,S
50 REM Y(T X 1)
55 MAT Y = X*B
56 MAT O = (1)*Y
57 PRINT "THE ESTIMATED MEAN VECTOR IS"
58 PRINT
59 MAT PRINT O
60 MAT Y = M*O
65 PRINT
66 PRINT
67 PRINT "THE ESTIMATED UTILITY VECTOR IS"
68 PRINT
69 MAT PRINT Y
75 REM A((T-1) X T)
76 MAT A = ZER
80 REM I=1 TO (T-1)
85 FOR I=1 TO 8
90 A(I,I) = 1
95 NEXT I
100 I1 = 1
105 REM I=2 TO T
110 FOR I=2 TO 9
115 IF Y(I1,1) > Y(I,1) THEN 125
120 I1 = I
125 NEXT I
130 REM IF I1=T
131 IF I1 = 9 THEN 145
135 REM A(I1,T)
136 A(I1,9) = 1
140 REM I=1 TO (T-1)
145 FOR I=1 TO 8
150 [illegible],I1) = -1
```

FIGURE 7--Continued

```
155 NEXT I
16Ø PRINT
161 PRINT
162 PRINT "THE A MATRIX IS"
163 PRINT
17Ø REM I=1 TO (T-1), J=1 TO T
175 FOR I=1 TO 8
18Ø FOR J=1 TO 9
185 PRINT USING 186, A(I,J)
186 FIELD ("-",F5.1)
19Ø NEXT J
191 PRINT
195 NEXT I
196 PRINT
197 PRINT
198 PRINT "I1 =";I1;"THIS IS THE NUMBER OF THE OPTIMAL TREAT."
2ØØ REM THIS ESTABLISHES THE A MATRIX.  IN WHAT FOLLOWS, D(I1,1)
2Ø1 REM IS THE DIFFERENCE BETWEEN THE LAST AND OPTIMAL TREATMENTS.
2Ø5 REM D((T-1) X 1)=THE VECTOR OF DIFFERENTIAL UTILITIES.
21Ø MAT D = A*Y
215 PRINT
216 PRINT
217 PRINT "THE DIFFERENTIAL UTILITY VECTOR IS"
218 PRINT
22Ø MAT PRINT D
221 PRINT
222 PRINT "(IN THE VECTOR ABOVE, THE";I1;"TH ELEMENT IS THE DU"
223 PRINT "BETWEEN THE OPTIMAL TREATMENT AND THE LAST TREATMENT"
225 PRINT "FROM THE VECTOR U.)"
23Ø REM L(T,T),C(T,(T-1)),W(P,T),T(T-1,T)P(T-1,T),R(T,P)
235 MAT W = TRN(X)
25Ø MAT P = A*M
255 MAT R = X*S
26Ø MAT C = TRN(P)
265 MAT M = R*W
27Ø MAT A = P*M
275 REM V((T-1) X (T-1))= COVARIANCE MATRIX OF D
28Ø MAT V = A*C
285 PRINT
286 PRINT
287 PRINT "THE COVARIANCE MATRIX OF D IS"
288 PRINT
29Ø REM I AND J=1 TO (T-1)
292 FOR I = 1 TO 8
294 FOR J = 1 TO 8
296 PRINT USING 215Ø, V(I,J)
298 NEXT J
```

FIGURE 7---Continued

```
300 PRINT
302 NEXT I
310 GOSUB 2000
320 PRINT
321 PRINT
325 PRINT "THE EXPECTED LOSS(BASED ON";N1;"DRAWS) OF CHOOSING"
326 PRINT "THE";I1;"TH TREATMENT IS";T1;".  THE C.V. IS";C1
340 REM THIS SECTION COMPUTES THE PREPOSTERIOR COVARIANCE OF D
345 READ S2
350 REM F(OBS/RUN X P)=THE DATA MATRIX FOR ONE EXP RUN
355 MAT READ F
360 REM G(P X 0/R)
365 MAT G = TRN(F)
370 MAT H = G*F
375 MAT S = INV(H)
380 MAT S = (S2)*S
385 MAT R = X*S
390 MAT M = R*W
395 MAT A = P*M
400 MAT Q = A*C
401 REM Q((T-1),(T-1))= THE EXPERIMENTAL COV. MAT. OF D
402 REM V FROM ABOVE = THE PRIOR COV. MAT. OF D
405 PRINT
406 PRINT
410 PRINT "THE EXPER. COV. MAT. OF D IS"
411 PRINT
412 REM I AND J = 1 TO (T-1)
413 FOR I = 1 TO 8
414 FOR J = 1 TO 8
415 PRINT USING 2150, Q(I,J)
416 NEXT J
417 PRINT
418 NEXT I
435 MAT U = INV(Q)
436 REM E((T-1),(T-1)),U((T-1),(T-1))
440 MAT E = INV(V)
445 MAT E = E + U
446 MAT Q = INV(E)
447 REM Q IS NOW THE POSTERIOR COV. MAT. OF D
448 PRINT
449 PRINT
450 PRINT "THE POSTERIOR COV. MAT. OF D IS"
451 PRINT
452 REM I AND J = 1 TO (T-1)
453 FOR I = 1 TO 8
454 FOR J = 1 TO 8
455 PRINT USING 2150, Q(I,J)
```

FIGURE 7--Continued

```
456 NEXT J
457 PRINT
458 NEXT I
459 MAT V = V - Q
46Ø REM V BELOW IS THE PREPOSTERIOR COV. MATRIX OF D
461 PRINT
462 PRINT
463 PRINT "THE PREPOSTERIOR COV. MAT. OF D IS"
464 PRINT
465 REM I AND J = 1 TO (T-1)
466 FOR I = 1 TO 8
467 FOR J = 1 TO 8
468 PRINT USING 215Ø, V(I,J)
469 NEXT J
47Ø PRINT
471 NEXT I
472 GOSUB 2ØØØ
475 PRINT
476 PRINT
48Ø PRINT "THE EXPECTED VALUE OF SAMPLE INFORMATION FOR"
481 PRINT "REPEATING THE EXPERIMENT ONCE IS";T1;".  THIS"
482 PRINT "ESTIMATE IS BASED ON";N1;"DRAWS.  THE C.V. IS";C1
5ØØ STOP
2ØØØ REM HERE THE MATRIX NECESSARY IN THE TRANSFORMATION FROM
2ØØ1 REM SPHERICAL NORMAL TO A NORMAL WITH COVARIANCE MATRIX
2ØØ2 REM V IS FOUND.
2ØØ3 REM E((T-1) X (T-1))
2ØØ5 MAT E = ZER
2Ø1Ø REM I= 1 TO (T-1)
2Ø15 FOR I=1 TO 8
2Ø2Ø E(I,1) = V(I,1)/(SQR(V(1,1)))
2Ø25 NEXT I
2Ø3Ø REM I=2 TO [illegible]-1)
2Ø35 FOR I=2 TO [illegible]
2Ø4Ø FOR J= 2 TO [illegible]
2Ø45 Q1 = Ø
2Ø5Ø FOR K=1 TO [illegible]
2Ø55 Q1 = Q1 + [illegible]E(J,K)
2Ø6Ø NEXT K
2Ø65 E(I,J) = V([illegible] - Q1
2Ø7Ø IF (I-J) = [illegible] TO 2Ø85
2Ø75 E(I,J) = E([illegible])/E(J,J)
2Ø8Ø GO TO 2Ø9Ø
2Ø85 E(I,J) = SQR(E(I,J))
2Ø9Ø NEXT J
2Ø95 NEXT I
21ØØ PRINT
```

FIGURE 7--Continued

```
2101 PRINT
2102 PRINT "THE PROPER LOWER TRIANGULAR MATRIX IS"
2105 PRINT
2108 REM I AND J = 1 TO (T-1)
2110 FOR I=1 TO 8
2115 FOR J=1 TO 8
2120 PRINT USING 2150, E(I,J)
2125 NEXT J
2130 PRINT
2135 NEXT I
2145 REM FORMAT BELOW MUST ALLOW FOR PRINTING OF ALL COLUMNS
2146 REM ON ONE LINE.
2150 FIELD ("-",F9.0)
2160 REM THIS SECTION COMPUTES EXPECTED LOSS (EVPI OR EVSI)
2162 T2 = 0
2163 U2 = 0
2165 N1 = 0
2170 N1 = N1 + 1
2173 REM I = 1 TO (T-1)
2175 FOR I = 1 TO 8
2180 R1 = RND(X)
2182 R2 = RND(X)
2185 Z(I,1) = SQR(-2*(LOG(R1)))*(COS(44/7*R2))
2190 NEXT I
2195 REM Z((T-1) X 1) IS A VECTOR OF SPHERICAL NORMAL DEVIATES
2200 MAT N = E*Z
2205 MAT Z = D + N
2210 REM Z IS A VECTOR OF OBSERVATIONS FROM N(D,V)
2212 REM J=1 TO (T-1)
2215 FOR J = 1 TO 8
2220 IF Z(1,1) > Z(J,1) THEN 2230
2225 Z(1,1) = Z(J,1)
2230 NEXT J
2235 IF Z(1,1) > 0 THEN 2245
2240 Z(1,1) = 0
2245 T2 = T2 +Z(1,1)
2250 U2 = U2 + Z(1,1)↑2
2251 IF N1<10000 THEN 2170
2252 REM THE 10000 ABOVE IS ARBITRARY
2255 T1 = T2/N1
2260 U1 = (U2/N1 - T1*T1)/N1
2265 C1 = SQR(U1)/T1
2275 RETURN
```

FIGURE 8

INPUT DATA FOR THE NINE-WEEK ANALYSIS

```
3ØØØ DATA 1,Ø,Ø,Ø,Ø,Ø,Ø,Ø,Ø,1,Ø,1.81
3ØØ2 DATA 1,1,Ø,Ø,Ø,Ø,Ø,Ø,Ø,1,Ø,1.81
3ØØ4 DATA 1,Ø,1,Ø,Ø,Ø,Ø,Ø,Ø,1,Ø,1.81
3ØØ6 DATA 1,Ø,Ø,1,Ø,Ø,Ø,Ø,Ø,1,Ø,1.81
3ØØ8 DATA 1,1,Ø,1,Ø,1,Ø,Ø,Ø,1,Ø,1.81
3Ø1Ø DATA 1,Ø,1,1,Ø,Ø,Ø,1,Ø,1,Ø,1.81
3Ø12 DATA 1,Ø,Ø,Ø,1,Ø,Ø,Ø,Ø,1,Ø,1.81
3Ø14 DATA 1,1,Ø,Ø,1,Ø,1,Ø,Ø,1,Ø,1.81
3Ø16 DATA 1,Ø,1,Ø,1,Ø,Ø,Ø,1,1,Ø,1.81
3Ø18 DATA 224.436, Ø, Ø, Ø, Ø, Ø, Ø, Ø, Ø
3Ø2Ø DATA Ø, 3Ø6.856, Ø, Ø, Ø, Ø, Ø, Ø, Ø
3Ø22 DATA Ø, Ø, 382.936, Ø, Ø, Ø, Ø, Ø, Ø
3Ø24 DATA Ø, Ø, Ø, 2Ø8.586, Ø, Ø, Ø, Ø, Ø
3Ø26 DATA Ø, Ø, Ø, Ø, 291.ØØ6, Ø, Ø, Ø, Ø
3Ø28 DATA Ø, Ø, Ø, Ø, Ø, 367.Ø86, Ø, Ø, Ø
3Ø3Ø DATA Ø, Ø, Ø, Ø, Ø, Ø, 2Ø8.586, Ø, Ø
3Ø32 DATA Ø, Ø, Ø, Ø, Ø, Ø, Ø, 291.ØØ6, Ø
3Ø34 DATA Ø, Ø, Ø, Ø, Ø, Ø, Ø, Ø, 367.Ø86
3Ø35 DATA 5.48Ø, -1.158, -1.867, -3.Ø29, -1.463, 2.665
3Ø36 DATA 1.811, 1.779, .843, -2.242, -3.5Ø4, .Ø12
3Ø37 DATA 3.765, .Ø81, .471, -.659, -2.192, -.874
3Ø38 DATA 2.ØØ4, -.847, -.3Ø9, -1.Ø22, 1.573, -.Ø13
3Ø4Ø DATA .Ø81,2.663,2.434, 1.156, .6Ø5, -2.931
3Ø41 DATA -1.99, -2.Ø44, -2.353, -1.331, .39, Ø
3Ø43 DATA .471, 2.434, 4.37, 1.291, .551, -2.784
3Ø44 DATA -1.587, -3.496, -4.249, -2.4Ø7, .4Ø3, -.Ø13
3Ø46 DATA -.659, 1.156, 1.291, 1.99, 1.Ø22, -2.Ø44
3Ø47 DATA -1.13, -2.Ø71, -1.237, -.282, -.175, Ø
3Ø49 DATA -2.192, .6Ø5, .551, 1.Ø22, 2.595, -.2Ø2
3Ø5Ø DATA -2.622, -.255, -1.425, .229, -1.13, .Ø13
3Ø52 DATA -.874, -2.931, -2.784, -2.Ø44, -.2Ø2, 5.11
3Ø53 DATA 1.614, 3.375, 2.676, 1.56, -.928, .Ø13
3Ø55 DATA 2.ØØ4, -1.99Ø, -1.587, -1.13, -2.622, 1.614
3Ø56 DATA 5.325, .793, 2.434, .78, 1.224, Ø
3Ø58 DATA -.847, -2.Ø44, -3.496, -2.Ø71, -.255, 3.375
3Ø59 DATA .793, 4.962, 3.375, 1.493, -.847, .Ø13
3Ø61 DATA -.3Ø9, -2.353,-4.249, -1.237, -1.425, 2.676
3Ø62 DATA 2.434, 3.375, 5.957, 2.326, -.377, .Ø13
3Ø64 DATA -1.Ø22, -1.331, -2.4Ø7, -.282, .229, 1.56
3Ø65 DATA .78, 1.493, 2.326, 2.259, -.188, Ø
3Ø67 DATA 1.573, .39, .4Ø3, -.175, -1.13, -.928
3Ø68 DATA 1[illegible], -.847, -.377, -.188, 1.667, -.Ø13
3Ø7Ø DATA -[illegible] Ø, -.Ø13, Ø, .Ø13, .Ø13, Ø
```

FIGURE 8--Continued

```
3Ø71 DATA .Ø13, .Ø13, Ø, -.Ø13, Ø
3Ø76 DATA 13.447
3Ø78 DATA 1,1,Ø,Ø,1,Ø,1,Ø,Ø,Ø,Ø,2.22
3Ø8Ø DATA 1,Ø,Ø,Ø,1,Ø,Ø,Ø,Ø,Ø,1,2.14
3Ø82 DATA 1,Ø,Ø,1,Ø,Ø,Ø,Ø,Ø,Ø,1,3.47
3Ø84 DATA 1,Ø,1,Ø,Ø,Ø,Ø,Ø,Ø,1,Ø,2.Ø2
3Ø86 DATA 1,Ø,1,Ø,1,Ø,Ø,Ø,1,Ø,Ø,1.31
3Ø88 DATA 1,Ø,1,1,Ø,Ø,Ø,1,Ø,1,Ø,1.13
3Ø9Ø DATA 1,1,Ø,Ø,Ø,Ø,Ø,Ø,Ø,1,Ø,1.57
3Ø92 DATA 1,1,Ø,1,Ø,1,Ø,Ø,Ø,Ø,Ø,2.Ø5
3Ø94 DATA 1,1,Ø,Ø,Ø,Ø,Ø,Ø,Ø,Ø,Ø,2.43
3Ø96 DATA 1,1,Ø,Ø,1,Ø,1,Ø,Ø,Ø,Ø,1.98
3Ø98 DATA 1,1,Ø,1,Ø,1,Ø,Ø,Ø,Ø,1,1.82
4ØØØ DATA 1,Ø 1,Ø,Ø,Ø,Ø,Ø,Ø,1,Ø,1.84
4ØØ2 DATA 1,Ø,1,Ø,1,Ø,Ø,Ø,1,Ø,1,1.83
4ØØ4 DATA 1,Ø,1,1,Ø,Ø,Ø,1,Ø,Ø,1,2.44
4ØØ6 DATA 1,Ø,Ø,Ø,Ø,Ø,Ø,Ø,Ø,Ø,Ø,1.75
4ØØ8 DATA 1,Ø,Ø,Ø,Ø,Ø,Ø,Ø,Ø,Ø,Ø,1.56
4Ø1Ø DATA 1,Ø,Ø,Ø,1,Ø,Ø,Ø,Ø,Ø,Ø,.61
4Ø12 DATA 1,Ø,Ø,1,Ø,Ø,Ø,Ø,Ø,Ø,Ø,1.35
4Ø14 END
```

FIGURE 9

OUTPUT DATA FOR THE NINE-WEEK ANALYSIS

THE ESTIMATED MEAN VECTOR IS

3.25972
2.1Ø172
1.39272
.23Ø72
1.73772
.142719999
1.79672
2.44972
.77272

THE ESTIMATED UTILITY VECTOR IS

731.598518
644.925392
533.322625
48.1249619
5Ø5.686946
52.39Ø5139
374.77Ø637
712.883218
283.654693

THE A MATRIX IS

-1.Ø	Ø	Ø	Ø	Ø	Ø	Ø	Ø	1.Ø
-1.Ø	1.Ø	Ø	Ø	Ø	Ø	Ø	Ø	Ø
-1.Ø	Ø	1.Ø	Ø	Ø	Ø	Ø	Ø	Ø
-1.Ø	Ø	Ø	1.Ø	Ø	Ø	Ø	Ø	Ø
-1.Ø	Ø	Ø	Ø	1.Ø	Ø	Ø	Ø	Ø
-1.Ø	Ø	Ø	Ø	Ø	1.Ø	Ø	Ø	Ø
-1.Ø	Ø	Ø	Ø	Ø	Ø	1.Ø	Ø	Ø
-1.Ø	Ø	Ø	Ø	Ø	Ø	Ø	1.Ø	Ø

I1 = 1 THIS IS THE NUMBER OF THE OPTIMAL TREAT.

FIGURE 9--Continued

THE DIFFERENTIAL UTILITY VECTOR IS

-447.943824
-86.6731255
-198.275892
-683.473556
-225.911571
-679.208004
-356.82788
-18.7152996

(IN THE VECTOR ABOVE, THE 1 TH ELEMENT IS THE DU
BETWEEN THE OPTIMAL TREATMENT AND THE LAST TREATMENT
FROM THE VECTOR U.)

THE COVARIANCE MATRIX OF D IS

245991	12568	-31785	56309	85542	50767	75987	42613
12568	214238	214746	58755	39807	98170	6322	100441
-31785	214746	501752	74021	28075	156411	-18096	128631
56309	58755	74021	93791	56508	68737	54976	48267
85542	39807	28075	56508	139984	60522	62249	49215
50767	98170	156411	68737	60522	219643	47297	66412
75987	6322	-18096	54976	62249	47297	126715	5874
42613	100441	128631	48267	49215	66412	5874	219273

THE PROPER LOWER TRIANGULAR MATRIX IS

495	0	0	0	0	0	0	0
25	462	0	0	0	0	0	0
-64	468	527	0	0	0	0	0
113	120	46	253	0	0	0	0
172	76	6	108	304	0	0	0
102	206	125	103	49	370	0	0
153	5	-20	149	63	38	273	0
85	212	65	38	44	-2	-56	394

THE EXPECTED LOSS(BASED ON 10000 DRAWS) OF CHOOSING
THE 1 TH TREATMENT IS 377.148492 . THE C.V. IS 9.73856987E-3

FIGURE 9--Continued

THE EXPER. COV. MAT. OF D IS

2334768	-99883	-540224	209435	790939	679365	722379	259293
-99883	1190621	807211	454227	189212	296767	228509	349345
-540224	807211	2447157	584763	36892	399438	222377	245966
209435	454227	584763	750730	341186	417883	337721	345556
790939	189212	36892	341186	1126759	533839	513721	313797
679365	296767	399438	417883	533839	1623394	613266	191114
722379	228509	222377	337721	513721	613266	864749	217698
259293	349345	245966	345556	313797	191114	217698	1020313

THE POSTERIOR COV. MAT. OF D IS

220447	9638	-30170	47825	76387	47234	67907	37222
9638	172949	160217	51194	33465	74378	13652	73540
-30170	160217	392373	64324	22079	116362	-1227	86807
47825	51194	64324	82988	49049	59544	48092	41802
76387	33465	22079	49049	124113	53528	55448	42232
47234	74378	116362	59544	53528	187884	48235	48123
67907	13652	-1227	48092	55448	48235	107488	12556
37222	73540	86807	41802	42232	48123	12556	174974

THE PREPOSTERIOR COV. MAT. OF D IS

25543	2930	-1615	8484	9154	3532	8080	5391
2930	41288	54528	7560	6341	23791	-7329	26900
-1615	54528	109378	9696	5995	40049	-16868	41824
8484	7560	9696	10802	7458	9192	6883	6464
9154	6341	5995	7458	15871	6993	6801	6982
3532	23791	40049	9192	6993	31758	-937	18288
8080	-7329	-16868	6883	6801	-937	19227	-6682
5391	26900	41824	6464	6982	18288	-6682	44298

THE PROPER LOWER TRIANGULAR MATRIX IS

159	0	0	0	0	0	0	0
18	202	0	0	0	0	0	0
-10	270	190	0	0	0	0	0
53	32	7	82	0	0	0	0
57	26	-2	43	100	0	0	0
22	115	47	47	7	115	0	0
50	-40	-28	69	19	5	95	0
33	129	37	2	16	4	-26	154

FIGURE 9--Continued

```
THE EXPECTED VALUE OF SAMPLE INFORMATION FOR
REPEATING THE EXPERIMENT ONCE IS 1Ø7.686132 .  THIS
ESTIMATE IS BASED ON 1ØØØØ DRAWS.  THE C.V. IS 1.34245876E-2

 5ØØ,  NORMAL EXIT FROM PROG.
```

FIGURE 9--Continued

THE EXPER. COV. MAT. OF D IS

1167384	-49941	-270112	104717	395469	339682	361189	129646
-49941	595310	403605	227113	94606	148383	114254	174672
-270112	403605	1223578	292381	18446	199719	111188	122983
104717	227113	292381	375365	170593	208941	168860	172778
395469	94606	18446	170593	563379	266919	256860	156898
339682	148383	199719	208941	266919	811697	306633	95557
361189	114254	111188	168860	256860	306633	432374	108849
129646	174672	122983	172778	156898	95557	108849	510156

THE POSTERIOR COV. MAT. OF D IS

199979	7432	-28777	41370	69102	44124	61514	32948
7432	147038	129132	45554	28923	60297	15991	58601
-28777	129132	326930	57212	18123	93061	5911	64924
41370	45554	57212	74466	43289	52557	42612	37022
69102	28923	18123	43289	111526	48174	49921	37122
44124	60297	93061	52557	48174	165693	46527	37984
61514	15991	5911	42612	49921	46527	94154	14639
32948	58601	64924	37022	37122	37984	14639	146974

THE PREPOSTERIOR COV. MAT. OF D IS

46011	5136	-3008	14939	16439	6643	14472	9665
5136	67199	85613	13200	10884	37873	-9668	41839
-3008	85613	174821	16809	9951	63350	-24007	63707
14939	13200	16809	19324	13219	16179	12364	11245
16439	10884	9951	13219	28457	12348	12328	12092
6643	37873	63350	16179	12348	53949	770	28427
14472	-9668	-24007	12364	12328	770	32561	-8765
9665	41839	63707	11245	12092	28427	-8765	72299

THE PROPER LOWER TRIANGULAR MATRIX IS

214	0	0	0	0	0	0	0
23	258	0	0	0	0	0	0
-14	332	252	0	0	0	0	0
69	44	11	111	0	0	0	0
76	35	-2	57	134	0	0	0
30	143	62	61	11	156	0	0
67	-43	-33	90	25	7	127	0
45	157	46	4	21	5	-34	203

FIGURE 9--Continued

THE EXPECTED VALUE OF SAMPLE INFORMATION FOR
REPEATING THE EXPERIMENT ONCE IS 157.6Ø7169 . THIS
ESTIMATE IS BASED ON 1ØØØØ DRAWS. THE C.V. IS 1.26142679E-2

5ØØ, NORMAL EXIT FROM PROG.

FIGURE 9--Continued

THE EXPER. COV. MAT. OF D IS

777477	-33261	-179894	69741	263382	226228	240552	86344
-33261	396476	268801	151257	63007	98823	76093	116332
-179894	268801	814903	194726	12285	133012	74051	81906
69741	151257	194726	249993	113615	139155	112461	115070
263382	63007	12285	113615	375210	177768	171069	104494
226228	98823	133012	139155	177768	540590	204217	63641
240552	76093	74051	112461	171069	204217	287961	72493
86344	116332	81906	115070	104494	63641	72493	339764

THE POSTERIOR COV. MAT. OF D IS

183120	5754	-27495	36303	63136	41383	56285	29499
5754	128685	108732	41107	25456	50804	16521	48978
-27495	108732	282185	51645	15273	77575	9233	51587
36303	41107	51645	67542	38696	47015	38178	33274
63136	25456	15273	38696	101262	43868	45372	33151
41383	50804	77575	47015	43868	148775	44130	31471
56285	16521	9233	38178	45372	44130	84069	15085
29499	48978	51587	33274	33151	31471	15085	127198

THE PREPOSTERIOR COV. MAT. OF D IS

62871	6813	-4290	20005	22406	9383	19701	13114
6813	85552	106014	17647	14351	47365	-10199	51462
-4290	106014	219566	22376	12801	78835	-27330	77044
20005	17647	22376	26248	17811	21721	16797	14992
22406	14351	12801	17811	38722	16653	16877	16063
9383	47365	78835	21721	16653	70867	3167	34941
19701	-10199	-27330	16797	16877	3167	42646	-9210
13114	51462	77044	14992	16063	34941	-9210	92075

THE PROPER LOWER TRIANGULAR MATRIX IS

250	0	0	0	0	0	0	0
27	291	0	0	0	0	0	0
-17	365	292	0	0	0	0	0
79	53	14	129	0	0	0	0
89	40	-2	65	157	0	0	0
37	159	72	70	14	183	0	0
78	-42	-35	102	30	10	147	0
52	171	51	7	25	5	-38	234

FIGURE 9--Continued

```
THE EXPECTED VALUE OF SAMPLE INFORMATION FOR
REPEATING THE EXPERIMENT ONCE IS 188.64457 .  THIS
ESTIMATE IS BASED ON 1ØØØØ DRAWS.  THE C.V. IS 1.2Ø833711E-2

 5ØØ,  NORMAL EXIT FROM PROG.
```

FIGURE 9--Continued

THE EXPER. COV. MAT. OF D IS

583692	-2497Ø	-135Ø56	52358	197734	169841	18Ø594	64823
-2497Ø	297655	2Ø18Ø2	113556	473Ø3	74191	57127	87336
-135Ø56	2Ø18Ø2	611789	14619Ø	9223	99859	55594	61491
52358	113556	14619Ø	187682	85296	1Ø447Ø	8443Ø	86389
197734	473Ø3	9223	85296	281689	133459	12843Ø	78449
169841	74191	99859	1Ø447Ø	133459	4Ø5848	153316	47778
18Ø594	57127	55594	8443Ø	12843Ø	153316	216187	54424
64823	87336	61491	86389	78449	47778	54424	255Ø78

THE POSTERIOR COV. MAT. OF D IS

169Ø8Ø	4471	-263Ø9	3227Ø	58191	38977	51949	26692
4471	114894	943Ø6	37514	22733	43965	16312	42264
-263Ø9	943Ø6	249444	47164	13134	66551	1Ø784	42736
3227Ø	37514	47164	61847	34982	4254Ø	34564	3Ø262
58191	22733	13134	34982	92794	4Ø336	416Ø5	29985
38977	43965	66551	4254Ø	4Ø336	13537Ø	4166Ø	26943
51949	16312	1Ø784	34564	416Ø5	4166Ø	76139	14871
26692	42264	42736	3Ø262	29985	26943	14871	112444

THE PREPOSTERIOR COV. MAT. OF D IS

7691Ø	8Ø97	-5475	24Ø39	27351	11789	24Ø38	15921
8Ø97	99344	12Ø439	2124Ø	17Ø73	542Ø4	-9989	58176
-5475	12Ø439	2523Ø7	26856	1494Ø	89859	-2888Ø	85895
24Ø39	2124Ø	26856	31943	21525	26197	2Ø412	18ØØ5
27351	17Ø73	1494Ø	21525	47189	2Ø186	2Ø644	1923Ø
11789	542Ø4	89859	26197	2Ø186	84272	5637	39468
24Ø38	-9989	-2888Ø	2Ø412	2Ø644	5637	5Ø576	-8997
15921	58176	85895	18ØØ5	1923Ø	39468	-8997	1Ø6829

THE PROPER LOWER TRIANGULAR MATRIX IS

277	Ø	Ø	Ø	Ø	Ø	Ø	Ø
29	313	Ø	Ø	Ø	Ø	Ø	Ø
-19	385	321	Ø	Ø	Ø	Ø	Ø
86	59	17	143	Ø	Ø	Ø	Ø
98	45	-1	71	173	Ø	Ø	Ø
42	168	79	77	16	2Ø3	Ø	Ø
86	-39	-36	11Ø	33	12	162	Ø
57	18Ø	54	9	27	5	-41	255

FIGURE 9--Continued

```
THE EXPECTED VALUE OF SAMPLE INFORMATION FOR
REPEATING THE EXPERIMENT ONCE IS 21Ø.967199 .  THIS
ESTIMATE IS BASED ON 1ØØØØ DRAWS.  THE C.V. IS 1.17297Ø38E-2

  5ØØ,  NORMAL EXIT FROM PROG.
```

FIGURE 9--Continued

THE EXPER. COV. MAT. OF D IS

466953	-19976	-1Ø8Ø44	41887	158187	135873	144475	51858
-19976	238124	161442	9Ø845	37842	59353	457Ø1	69869
-1Ø8Ø44	161442	489431	116952	7378	79887	44475	49193
41887	9Ø845	116952	15Ø146	68237	83576	67544	69111
158187	37842	7378	68237	225351	1Ø6767	1Ø2744	62759
135873	59353	79887	83576	1Ø6767	324678	122653	38222
144475	457Ø1	44475	67544	1Ø2744	122653	172949	43539
51858	69869	49193	69111	62759	38222	43539	2Ø4Ø62

THE POSTERIOR COV. MAT. OF D IS

157Ø89	3464	-252ØØ	28965	53984	36832	48255	24348
3464	1Ø3965	83422	34517	2Ø518	3874Ø	15789	37248
-252ØØ	83422	224Ø22	43432	11457	58212	11454	36412
28965	34517	43432	57Ø38	31895	38818	31543	27759
53984	2Ø518	11457	31895	85628	3735Ø	38411	27371
36832	3874Ø	58212	38818	3735Ø	124316	39295	23574
48255	15789	11454	31543	38411	39295	69644	14385
24348	37248	36412	27759	27371	23574	14385	1ØØ855

THE PREPOSTERIOR COV. MAT. OF D IS

889Ø1	91Ø3	-6584	27344	31558	13934	27731	18265
91Ø3	11Ø272	131323	24237	19289	59429	-9467	63192
-6584	131323	27773Ø	3Ø588	16617	98198	-2955Ø	92219
27344	24237	3Ø588	36752	24613	29919	23433	2Ø5Ø8
31558	19289	16617	24613	54356	23171	23838	21843
13934	59429	98198	29919	23171	95326	8ØØ2	42837
27731	-9467	-2955Ø	23433	23838	8ØØ2	57Ø71	-8511
18265	63192	92219	2Ø5Ø8	21843	42837	-8511	118418

THE PROPER LOWER TRIANGULAR MATRIX IS

298	Ø	Ø	Ø	Ø	Ø	Ø	Ø
3Ø	33Ø	Ø	Ø	Ø	Ø	Ø	Ø
-22	399	343	Ø	Ø	Ø	Ø	Ø
91	64	19	154	Ø	Ø	Ø	Ø
1Ø5	48	-1	76	186	Ø	Ø	Ø
46	175	85	81	19	2[illegible]	Ø	Ø
93	-37	-36	117	36	[illegible]	174	Ø
61	185	56	11	29	[illegible]	-44	272

FIGURE 9--Continued

```
THE EXPECTED VALUE OF SAMPLE INFORMATION FOR
REPEATING THE EXPERIMENT ONCE IS 228.165289 .  THIS
ESTIMATE IS BASED ON 10000 DRAWS.  THE C.V. IS 1.14717399E-2

  500,  NORMAL EXIT FROM PROG.
```

FIGURE 9---Continued

THE EXPER. COV. MAT. OF D IS

389906	-16680	-90217	34975	132086	113453	120637	43301
-16680	198833	134804	75856	31598	49560	38161	58340
-90217	134804	408675	97655	6161	66706	37137	41076
34975	75856	97655	125371	56978	69786	56399	57707
132086	31598	6161	56978	188168	89151	85791	52404
113453	49560	66706	69786	89151	271106	102415	31916
120637	38161	37137	56399	85791	102415	144413	36355
43301	58340	41076	57707	52404	31916	36355	170392

THE POSTERIOR COV. MAT. OF D IS

146862	2676	-24178	26253	50407	34935	45110	22392
2676	95162	75000	32008	18704	34662	15152	33394
-24178	75000	203867	40310	10129	51762	11650	31743
26253	32008	40310	52979	29325	35717	29020	25671
50407	18704	10129	29325	79568	34823	35707	25205
34935	34662	51762	35717	34823	115132	37130	20997
45110	15152	11650	29020	35707	37130	64281	13801
22392	33394	31743	25671	25205	20997	13801	91606

THE PREPOSTERIOR COV. MAT. OF D IS

99128	9891	-7606	30056	35135	15831	30876	20221
9891	119076	139746	26746	21102	63507	-8829	67046
-7606	139746	297885	33711	17945	104649	-29746	96888
30056	26746	33711	40812	27183	33020	25955	22596
35135	21102	17945	27183	60416	25698	26542	24009
15831	63507	104649	33020	25698	104511	10167	45414
30876	-8829	-29746	25955	26542	10167	62434	-7927
20221	67046	96888	22596	24009	45414	-7927	127667

THE PROPER LOWER TRIANGULAR MATRIX IS

314	0	0	0	0	0	0	0
31	343	0	0	0	0	0	0
-24	408	360	0	0	0	0	0
95	69	21	162	0	0	0	0
111	51	-1.	79	197	0	0	0
50	180	89	85	20	232	0	0
98	-34	-36	121	38	15	183	0
64	189	58	13	31	4	-45	284

FIGURE 9--Continued

```
THE EXPECTED VALUE OF SAMPLE INFORMATION FOR
REPEATING THE EXPERIMENT ONCE IS 241.75357 .  THIS
ESTIMATE IS BASED ON 1ØØØØ DRAWS.  THE C.V. IS 1.1276310Ø6E-2

  5ØØ,  NORMAL EXIT FROM PROG.
```

FIGURE 10

INPUT DATA FOR THE TWELVE-WEEK ANALYSIS

```
3ØØØ DATA 1,Ø,Ø,Ø,Ø,Ø,Ø,Ø,Ø,1,Ø,1.81
3ØØ2 DATA 1,1,Ø,Ø,Ø,Ø,Ø,Ø,Ø,1,Ø,1.81
3ØØ4 DATA 1,Ø,1,Ø,Ø,Ø,Ø,Ø,Ø,1,Ø,1.81
3ØØ6 DATA 1,Ø,Ø,1,Ø,Ø,Ø,Ø,Ø,1,Ø,1.81
3ØØ8 DATA 1,1,Ø,1,Ø,1,Ø,Ø,Ø,1,Ø,1.81
3Ø1Ø DATA 1,Ø,1,1,Ø,Ø,Ø,1,Ø,1,Ø,1.81
3Ø12 DATA 1,Ø,Ø,Ø,1,Ø,Ø,Ø,Ø,1,Ø,1.81
3Ø14 DATA 1,1,Ø,Ø,1,Ø,1,Ø,Ø,1,Ø,1.81
3Ø16 DATA 1,Ø,1,Ø,1,Ø,Ø,Ø,1,1,Ø,1.81
3Ø18 DATA 224.436, Ø, Ø, Ø, Ø, Ø, Ø, Ø, Ø
3Ø2Ø DATA Ø, 3Ø6.856, Ø, Ø, Ø, Ø, Ø, Ø, Ø
3Ø22 DATA Ø, Ø, 382.936, Ø, Ø, Ø, Ø, Ø, Ø
3Ø24 DATA Ø, Ø, Ø, 2Ø8.586, Ø, Ø, Ø, Ø, Ø
3Ø26 DATA Ø, Ø, Ø, Ø, 291.ØØ6, Ø, Ø, Ø, Ø
3Ø28 DATA Ø, Ø, Ø, Ø, Ø, 367.Ø86, Ø, Ø, Ø
3Ø3Ø DATA Ø, Ø, Ø, Ø, Ø, Ø, 2Ø8.586, Ø, Ø
3Ø32 DATA Ø, Ø, Ø, Ø, Ø, Ø, Ø, 291.ØØ6, Ø
3Ø34 DATA Ø, Ø, Ø, Ø, Ø, Ø, Ø, Ø, 367.Ø86
3Ø35 DATA 5.688, -1.871, -2.5Ø7, -3.Ø12, -1.858, 2.9Ø4
3Ø36 DATA 2.543, 1.6ØØ, 1.387, -1.369, -2.36Ø, .ØØ8
3Ø37 DATA 2.41, -.23, .2Ø9, -.463, -1.448, -.463
3Ø38 DATA 1.344, -.475, -.Ø81, -.637, .985, -.Ø12
3Ø4Ø DATA -.23, 1.7-9, 1.61Ø, .823, .452, -1.923
3Ø41 DATA -1.321, -1.39Ø, -1.552, -.834, .243, Ø
3Ø43 DATA .2Ø9, 1.61Ø, 2.828, .9Ø4, .417, -1.842
3Ø44 DATA -1.Ø66, -2.317, -2.746, -1.5Ø6, .255, Ø
3Ø46 DATA -.463, .823, .9Ø4, 1.344, .7Ø7, -1.39
3Ø47 DATA -.776, -1.4Ø2, -.88Ø, -.197, .1Ø4, Ø
3Ø49 DATA -1.448, .452, .417, .7Ø7, 1.7Ø3, -.2Ø9, -1.715
3Ø5Ø DATA -.232, -.996, .151, -.7Ø7, Ø
3Ø52 DATA -.463, -1.923, -1.842, -1.39, -.2Ø9, 3.3Ø2
3Ø53 DATA 1.1Ø1, 2.236, 1.749, .985, -.568, .Ø12
3Ø55 DATA 1.344, -1.321, -1.Ø66, -.776, -1.715, 1.1Ø1
3Ø56 DATA 3.464, .568, 1.622, .487, .776, Ø
3Ø58 DATA -.475, -1.39, -2.317, -1.4Ø2, -.232, 2.236
3Ø59 DATA .568, 3.255, 2.224, .962, -.544, .Ø12
3Ø61 DATA -.Ø81, -1.552, -2.746, -.88, -.996, 1.749
3Ø62 DATA 1.622, 2.224, 3.893, 1.448, -.2Ø9, Ø
3Ø64 DATA -.637, -.834, -1.5Ø6, -.197, .151, .985
3Ø65 DATA .487, .962, 1.448, 1.413, -.116, Ø
3Ø67 DATA .985, .243, .255, .1Ø4, -.7Ø7, -.568
3Ø68 DATA .776, -.544, -.2Ø9, -.116, 1.Ø54, -.Ø12
3Ø7Ø DATA -.Ø12, Ø, Ø, Ø, Ø, .Ø12, Ø
```

FIGURE 10--Continued

```
3Ø71 DATA .Ø12, Ø, Ø, -.Ø12, Ø
3Ø76 DATA 11.585
3Ø78 DATA 1,1,Ø,Ø,1,Ø,1,Ø,Ø,Ø,Ø,2.22
3Ø8Ø DATA 1,Ø,Ø,Ø,1,Ø,Ø,Ø,Ø,Ø,1,2.14
3Ø82 DATA 1,Ø,Ø,1,Ø,Ø,Ø,Ø,Ø,Ø,1,3.47
3Ø84 DATA 1,Ø,1,Ø,Ø,Ø,Ø,Ø,Ø,1,Ø,2.Ø2
3Ø86 DATA 1,Ø,1,Ø,1,Ø,Ø,Ø,1,Ø,Ø,1.31
3Ø88 DATA 1,Ø,1,1,Ø,Ø,Ø,1,Ø,1,Ø,1.13
3Ø9Ø DATA 1,1,Ø,Ø,Ø,Ø,Ø,Ø,Ø,1,Ø,1.57
3Ø92 DATA 1,1,Ø,1,Ø,1,Ø,Ø,Ø,Ø,Ø,2.Ø5
3Ø94 DATA 1,1,Ø,Ø,Ø,Ø,Ø,Ø,Ø,Ø,Ø,2.43
3Ø96 DATA 1,1,Ø,Ø,1,Ø,1,Ø,Ø,Ø,Ø,1.98
3Ø98 DATA 1,1,Ø,1,Ø,1,Ø,Ø,Ø,Ø,1,1.82
4ØØØ DATA 1,Ø,1,Ø,Ø,Ø,Ø,Ø,Ø,1,Ø,1.84
4ØØ2 DATA 1,Ø,1,Ø,1,Ø,Ø,Ø,1,Ø,1,1.83
4ØØ4 DATA 1,Ø,1,1,Ø,Ø,Ø,1,Ø,Ø,1,2.44
4ØØ6 DATA 1,Ø,Ø,Ø,Ø,Ø,Ø,Ø,Ø,Ø,Ø,1.75
4ØØ8 DATA 1,Ø,Ø,Ø,Ø,Ø,Ø,Ø,Ø,Ø,Ø,1.56
4Ø1Ø DATA 1,Ø,Ø,Ø,1,Ø,Ø,Ø,Ø,Ø,Ø,.61
4Ø12 DATA 1,Ø,Ø,1,Ø,Ø,Ø,Ø,Ø,Ø,Ø,1.35
4Ø14 END
```

FIGURE 11

OUTPUT DATA FOR THE TWELVE-WEEK ANALYSIS

THE ESTIMATED MEAN VECTOR IS

4.33348
2.46248
1.82647999
1.32147999
2.35448
.414479999
2.47548
3.14748
1.35547999

THE ESTIMATED UTILITY VECTOR IS

972.588917
755.626762
699.424945
275.642227
685.1678Ø6
152.1498Ø5
516.35Ø471
915.935564
497.577731

THE A MATRIX IS

-1.Ø	Ø	Ø	Ø	Ø	Ø	Ø	Ø	1.Ø
-1.Ø	1.Ø	Ø	Ø	Ø	Ø	Ø	Ø	Ø
-1.Ø	Ø	1.Ø	Ø	Ø	Ø	Ø	Ø	Ø
-1.Ø	Ø	Ø	1.Ø	Ø	Ø	Ø	Ø	Ø
-1.Ø	Ø	Ø	Ø	1.Ø	Ø	Ø	Ø	Ø
-1.Ø	Ø	Ø	Ø	Ø	1.Ø	Ø	Ø	Ø
-1.Ø	Ø	Ø	Ø	Ø	Ø	1.Ø	Ø	Ø
-1.Ø	Ø	Ø	Ø	Ø	Ø	Ø	1.Ø	Ø

IJ = 1 THIS IS THE NUMBER OF THE OPTIMAL TREAT.

FIGURE 11--Continued

THE DIFFERENTIAL UTILITY VECTOR IS

-475.Ø11186
-216.962154
-273.163972
-696.94669
-287.42111
-82Ø.439112
-456.238446
-56.6533524

(IN THE VECTOR ABOVE, THE 1 TH ELEMENT IS THE DU
BETWEEN THE OPTIMAL TREATMENT AND THE LAST TREATMENT
FROM THE VECTOR U.)

THE COVARIANCE MATRIX OF D IS

161532	32Ø8	-15444	378Ø7	45121	32845	48945	22549
32Ø8	127887	129231	43232	21657	56552	8534	57881
-15444	129231	32Ø199	51964	13819	1ØØ442	-7994	76Ø18
378Ø7	43232	51964	63468	4Ø799	479ØØ	37859	364Ø4
45121	21657	13819	4Ø799	852Ø2	348ØØ	42455	29Ø36
32845	56552	1ØØ442	479ØØ	348ØØ	141457	32469	37Ø11
48945	8534	-7994	37859	42455	32469	83299	8499
22549	57881	76Ø18	364Ø4	29Ø36	37Ø11	8499	138597

THE PROPER LOWER TRIANGULAR MATRIX IS

4Ø1	Ø	Ø	Ø	Ø	Ø	Ø	Ø
7	357	Ø	Ø	Ø	Ø	Ø	Ø
-38	362	432	Ø	Ø	Ø	Ø	Ø
94	118	28	199	Ø	Ø	Ø	Ø
112	58	-6	118	234	Ø	Ø	Ø
81	156	1Ø8	92	26	298	Ø	Ø
121	21	-25	123	54	3Ø	219	Ø
56	16Ø	46	53	31	-11	-38	319

THE EXPECTED LOSS(BASED ON 1ØØØØ DRAWS) OF CHOOSING
THE 1 TH TREATMENT IS 23Ø.Ø13Ø68 . THE C.V. IS 1.17Ø96Ø43E-2

FIGURE 11--Continued

THE EXPER. COV. MAT. OF D IS

2011473	-86052	-465420	180434	681418	585293	622351	223388
-86052	1025756	695437	391331	163012	255674	196867	300971
-465420	695437	2108300	503791	31783	344128	191584	211907
180434	391331	503791	646777	293942	360019	290957	297707
681418	163012	31783	293942	970737	459918	442586	270345
585293	255674	344128	360019	459918	1398603	528347	164651
622351	196867	191584	290957	442586	528347	745007	187554
223388	300971	211907	297707	270345	164651	187554	879031

THE POSTERIOR COV. MAT. OF D IS

148030	2327	-15384	33374	41851	30939	44825	20576
2327	110768	105357	38483	19783	46501	10875	47050
-15384	105357	266487	45997	13051	81078	-201	57764
33374	38483	45997	57500	36499	42618	34115	32129
41851	19783	13051	36499	77799	32292	38106	26482
30939	46501	81078	42618	32292	125910	32416	29467
44825	10875	-201	34115	38106	32416	73365	10511
20576	47050	57764	32129	26482	29467	10511	117928

THE PREPOSTERIOR COV. MAT. OF D IS

13501	880	-60	4432	3269	1906	4119	1972
880	17118	23874	4748	1874	10050	-2340	10830
-60	23874	53712	5967	768	19364	-7793	18254
4432	4748	5967	5968	4299	5282	3744	4274
3269	1874	768	4299	7402	2507	4349	2554
1906	10050	19364	5282	2507	15546	52	7544
4119	-2340	-7793	3744	4349	52	9934	-2012
1972	10830	18254	4274	2554	7544	-2012	20668

THE PROPER LOWER TRIANGULAR MATRIX IS

116	0	0	0	0	0	0	0
7	130	0	0	0	0	0	0
-1.	182	142	0	0	0	0	0
38	34	-1	57	0	0	0	0
28	12	-10	47	63	0	0	0
16	75	38	36	-4	81	0	0
35	-19	-28	52	12	2	67	0
16	81	23	15	8	-4	-17	111

FIGURE 11--Continued

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THE EXPECTED VALUE OF SAMPLE INFORMATION FOR
REPEATING THE EXPERIMENT ONCE IS 39.5279211 .  THIS
ESTIMATE IS BASED ON 1ØØØØ DRAWS.  THE C.V. IS 1.84722665E-2

 5ØØ,  NORMAL EXIT FROM PROG.
```

FIGURE 11--Continued

THE EXPER. COV. MAT. OF D IS

1ØØ5736	-43Ø26	-23271Ø	9Ø217	34Ø7Ø9	292646	311175	111694
-43Ø26	512878	347718	195665	815Ø6	127837	98433	15Ø485
-23271Ø	347718	1Ø5415Ø	251895	15891	172Ø64	95792	1Ø5953
9Ø217	195665	251895	323388	146971	18ØØØ9	145478	148853
34Ø7Ø9	815Ø6	15891	146971	435368	229959	221293	135172
292646	127837	172Ø64	18ØØØ9	229959	6993Ø1	264173	82325
311175	98433	95792	145478	221293	264173	3725Ø3	93777
111694	15Ø485	1Ø5953	148853	135172	82325	93777	439515

THE POSTERIOR COV. MAT. OF D IS

136755	1646	-15237	29788	39Ø79	29255	414Ø5	18898
1646	98352	8945Ø	34766	18Ø86	39632	1171Ø	39871
-15237	8945Ø	23ØØ43	41464	12Ø61	68Ø93	3843	46375
29788	34766	41464	5259Ø	32975	38417	3Ø963	28832
39Ø79	18Ø86	12Ø61	32975	71637	3ØØ58	34658	24251
29255	39632	68Ø93	38417	3ØØ58	114ØØ4	31472	24579
414Ø5	1171Ø	3843	3Ø963	34658	31472	65875	11156
18898	39871	46375	28832	24251	24579	11156	1Ø3Ø43

THE PREPOSTERIOR COV. MAT. OF D IS

24777	1561	-2Ø7	8Ø19	6Ø41	359Ø	754Ø	3651
1561	29535	39781	8465	357Ø	16919	-3175	18Ø1Ø
-2Ø7	39781	9Ø156	1Ø5ØØ	1758	32349	-11837	29643
8Ø19	8465	1Ø5ØØ	1Ø878	7823	9483	6896	7571
6Ø41	357Ø	1758	7823	13565	4741	7796	4784
359Ø	16919	32349	9483	4741	27452	996	12431
754Ø	-3175	-11837	6896	7796	996	17424	-2657
3651	18Ø1Ø	29643	7571	4784	12431	-2657	35553

THE PROPER LOWER TRIANGULAR MATRIX IS

157	Ø	Ø	Ø	Ø	Ø	Ø	Ø
9	171	Ø	Ø	Ø	Ø	Ø	[illegible]
-1	231	19Ø	Ø	Ø	Ø	Ø	
5Ø	46	-1	78	Ø	Ø	Ø	
38	18	-13	63	86	Ø	Ø	
22	97	51	49	-4	111	Ø	[illegible]
47	-21	-35	69	17	4	91	Ø
23	1Ø3	29	2Ø	12	-5	-23	149

FIGURE 11--Continued

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THE EXPECTED VALUE OF SAMPLE INFORMATION FOR
REPEATING THE EXPERIMENT ONCE IS 67.696449 .  THIS
ESTIMATE IS BASED ON 1ØØØØ DRAWS.  THE C.V. IS 1.65912669E-2

 5ØØ,  NORMAL EXIT FROM PROG.
```

FIGURE 11--Continued

THE EXPER. COV. MAT. OF D IS

669820	-28655	-154984	60084	226912	194902	207243	74388
-28655	341576	231580	130313	54283	85139	65557	100223
-154984	231580	702064	167762	10584	114594	63797	70565
60084	130313	167762	215376	97882	119886	96888	99136
226912	54283	10584	97882	323255	153152	147381	90025
194902	85139	114594	119886	153152	465735	175939	54828
207243	65557	63797	96886	147381	175939	248087	62455
74388	100223	70565	99136	90025	54828	62455	292717

THE POSTERIOR COV. MAT. OF D IS

127153	1114	-15024	26826	36683	27753	38503	17457
1114	80741	77961	31742	16593	34576	11873	34704
-15024	77961	203274	37840	11067	58687	6059	38610
26826	31742	37840	48461	30033	34966	28293	26178
36683	16593	11067	30033	66403	28088	31828	22324
27753	34576	58687	34966	28088	104413	30216	21129
38503	11873	6059	28293	31828	30216	59920	11210
17457	34704	38610	26178	22324	21129	11210	91661

THE PREPOSTERIOR COV. MAT. OF D IS

34379	2093	-420	10981	8437	5092	10442	5091
2093	39146	51269	11489	5063	21975	-3338	23177
-420	51269	116925	14124	2752	41755	-14053	37407
10981	11489	14124	15006	10765	12934	9566	10225
8437	5063	2752	10765	18799	6711	10627	6712
5092	21975	41755	12934	6711	37044	2252	15881
10442	-3338	-14053	9566	10627	2252	23378	-2711
5091	23177	37407	10225	6712	15881	-2711	46936

THE PROPER LOWER TRIANGULAR MATRIX IS

185	0	0	0	0	0	0	0
11	197	0	0	0	0	0	0
-2	259	222	0	0	0	0	0
59	54	0.	92	0	0	0	0
45	23	-14	73	102	0	0	0
27	109	59	57	-4	131	0	0
56	-20	-39	7[illegible]	20	5	107	0
27	115	33	2[illegible]	1[illegible]	-6	-2[illegible]	173

FIGURE 11--Continued

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THE EXPECTED VALUE OF SAMPLE INFORMATION FOR
REPEATING THE EXPERIMENT ONCE IS 86.636Ø153 .  THIS
ESTIMATE IS BASED ON 1ØØØØ DRAWS.  THE C.V. IS 1.5697824E-2

 5ØØ,  NORMAL EXIT FROM PROG.
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FIGURE 11--Continued

THE EXTER. COV. MAT. OF D IS

502868	-21513	-116355	451Ø8	17Ø354	146323	155587	55847
-21513	256439	173859	97832	4Ø753	63918	49216	75242
-116355	173859	527Ø75	125947	7945	86Ø32	47896	52976
451Ø8	97832	125947	161694	73485	9ØØØ4	72739	74426
17Ø354	4Ø753	7945	73485	242684	114979	11Ø646	67586
146323	63918	86Ø32	9ØØØ4	114979	34965Ø	132Ø86	41162
155587	49216	47896	72739	11Ø646	132Ø86	186251	46888
55847	75242	52976	74426	67586	41162	46888	219757

THE POSTERIOR COV. MAT. OF D IS

118934	697	-14767	24365	346Ø5	26416	36Ø24	16221
697	81Ø62	69292	29241	153Ø2	3Ø7Ø8	11716	3Ø815
-14767	69292	182761	34877	1Ø148	51585	7279	33Ø41
24365	29241	34877	44968	27564	32Ø97	26Ø32	24ØØ3
346Ø5	153Ø2	1Ø148	27564	61933	26365	29471	2Ø67Ø
26416	3Ø7Ø8	51585	32Ø97	26365	96513	28892	18574
36Ø24	11716	7279	26Ø32	29471	28892	55Ø72	1Ø995
16221	3Ø815	33Ø41	24ØØ3	2Ø67Ø	18574	1Ø995	82694

THE PREPOSTERIOR COV. MAT. OF D IS

42598	251Ø	-677	13442	1Ø516	6429	12921	6328
251Ø	46825	59939	1399Ø	6355	25843	-3181	27Ø66
-677	59939	137438	17Ø87	367Ø	48857	-15273	42977
13442	1399Ø	17Ø87	185ØØ	13234	158Ø3	11827	124ØØ
1Ø516	6355	367Ø	13234	23268	8434	12984	8365
6429	25843	48857	158Ø3	8434	44944	3576	18436
12921	-3181	-15273	11827	12984	3576	28227	-2496
6328	27Ø66	42977	124ØØ	8365	18436	-2496	559Ø2

THE PROPER LOWER TRIANGULAR MATRIX IS

2Ø6	Ø	Ø	Ø	Ø	Ø	Ø	Ø
12	216	Ø	Ø	Ø	Ø	Ø	Ø
-3	277	245	Ø	Ø	Ø	Ø	Ø
65	61	1	1Ø2	Ø	Ø	Ø	Ø
5Ø	26	-14	81	114	Ø	Ø	Ø
31	117	66	63	-3	147	Ø	Ø
62	-18	-4Ø	86	23	7	119	Ø
3Ø	123	35	27	15	-7	-28	191

FIGURE 11--Continued

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THE EXPECTED VALUE OF SAMPLE INFORMATION FOR
REPEATING THE EXPERIMENT ONCE IS 1ØØ.916967 .  THIS
ESTIMATE IS BASED ON 1ØØØØ DRAWS.  THE C.V. IS 1.513Ø3154E-2

 5ØØ,  NORMAL EXIT FROM PROG.
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FOOTNOTE

[1] Jolayne Service, <u>A User's Guide to the Statistical Analysis System</u> (Raleigh, N. C.: North Carolina State University Press, 1972), pp. 94-120.

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